Pricing Dynamics of Post-IPO Selldowns: How Sponsor-Backing Affects the Pricing, Performance and Timing of Selldowns

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Abstract

This paper examines the effect of financial sponsor-backing on post-IPO accelerated selldowns, an increasingly popular exit strategy. Selldowns are defined as block trades offered by pre-IPO investors that are announced after market close and priced and allocated to institutional investors before opening the next trading day. Using a sample of 1,232 developed-market selldowns taking place between 2000 and 2016, we find that sponsor-backed selldowns have smaller discounts and greater 1-year abnormal returns compared to non-sponsored selldowns, which is in line with previous studies on IPOs. Further, we show that selldowns where pre-IPO sponsors sell their last remaining share (clean-up trade) and eliminate the share overhang have smaller discounts and less negative first-day returns, which we attribute to the market reacting positively to the removal of the overhang. The clean-up effect is shown to explain a large part of the difference in discount between sponsor-backed and non-sponsored selldowns. Additionally, our results suggest that the positive 1-year abnormal performance found in sponsor-backed selldowns decreases significantly after the clean-up trade, which supports the finding that sponsor-backing enhances aftermarket performance. Finally, we find that sponsors do selldowns at a shorter time from expiry of the lock-up provision. We also present evidence suggesting that sponsors put less emphasis on the increase in valuation after the IPO than non-sponsors do when the timing of a selldown is determined. Non-sponsors are more likely to do a selldown shortly after expiry when valuation has increased markedly since the IPO, but we find no evidence that markets react negatively to the signaling value when they do.

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1 Introduction

Underpricing and long-run performance of initial public offerings (IPOs) are topics extensively covered within the field of financial economics. While IPOs are economically significant events, larger IPOs often involve further seasoned equity offerings (SEOs). Of these seasoned offerings, the popularity of the accelerated bookbuilding has increased significantly in the past two decades (Bortolotti et al., 2008). This paper focuses on seasoned equity offerings specifically as selldowns in recently listed companies. Selldowns are defined as overnight accelerated bookbuilt offerings of a block of shares by pre-IPO investors. In other words, a transaction that occurs when the pre-IPO investor is looking to dispose of a larger position in a given company and does so by offering it to institutional investors while markets are closed.

The aggregate value of these post-IPO transactions represents material economic events. While SEOs in general is a somewhat covered topic, we argue that selldowns deserve to be examined independently. The special nature of the transaction, where the company itself is not an agent in the offering, makes the pricing dynamics very different. When considering other seasoned equity offerings, the use of proceeds will be integral in the pricing effect, depending on market participant's assessment of the intended use. Transactions where no capital is raised for the company itself allows for the isolation of price and aftermarket performance effects relating to different types of owners selling shares in a company. The increasing use of accelerated bookbuildings as a means of selling remaining stakes in a company have made selldowns a key part of the exit strategy when owners take companies public (Schöber, 2008). Because of the economic significance of selldowns, even investors that do not partake in selldowns may benefit from knowledge on the subject. The objective of this paper is to study the effects of sponsor-backing on the pricing, aftermarket performance and timing of selldowns. Additionally, we aim to set this in relation to previous literature on IPOs, SEOs and sponsor-backing in order to find possible explanations for the effects we find. We do this by evaluating the impact of ex-ante observable factors on pricing dynamics and timing of selldowns. Specifically, we examine effects of sponsor-backing on three aspects of selldowns: discounts, share price performance post-selldown and timing from expiry of the lock-up provision (a period in which the pre-IPO investor commits to not selling shares in the company). Further, we aim to expand the knowledge of a topic that has received little attention from researchers despite its large and growing position in equity capital markets. We take on a global perspective, using a sample of selldowns in companies in all developed markets from 2000-2016.

1.1 Problem statement

This paper seeks to investigate pricing dynamics, aftermarket performance and timing of selldowns. By taking a quantitative empirical approach we aim to find effects of sponsor-backing in relation to these three aspect. Our main research question is:

How does sponsor-backing affect the pricing, aftermarket performance and timing of selldowns?

We answer the main research question through an examination of the following three sub-questions:

1. How does pricing and aftermarket performance of selldowns differ between sponsor-backed and non-sponsored transactions, and are these relationships similar to those previously found in IPOs and SEOs?

2. How does clean-up trades differ from other sponsor-backed selldowns in terms of pricing and aftermarket performance?

3. How does the selldown timing after lock-up expiry differ between sponsors and non-sponsors, and how does the market reaction to announcements of selldowns immediately after lock-up expiry differ for the two?

1.2 Definitions

To the best of our knowledge, no previous research has focused directly on the topic of selldowns. Thus, no consensus definition from the literature can be relied upon. For the purpose of this paper, a selldown is defined as the moment a pre-IPO investor sells a block of shares that has been retained in the initial offering. We require selldowns to take place as an accelerated bookbuilding, where announcement and pricing take place overnight to isolate the effects we are considering throughout this paper. The accelerated bookbuilding is a special type of share sale, where a bookrunner (typically a larger bank with equity capital markets offerings) is mandated by the seller to sell a block of shares, taking place overnight for swift execution with limited price impact (Ramirez, 2011). This implies that there is no prolonged marketing phase leading up to the transaction, and there is no trading taking place between the announcement and closing of the transaction, effectively isolating the selldown effect on discounts and first-day returns.

We define a sponsor-backed transaction as one where a private equity firm or a venture capital firm has significant holdings. In practice, we rely on Dealogic's identification of sponsor-backed deals. Generally, financial sponsors of this sort can be split in private equity and venture capital. Venture capital makes up only a small part of the sample with a total of 54 out of 1,232 selldowns. Thus, we focus mainly on private equity throughout this paper.

While terms are often used interchangeably, this paper will use the term underpricing for IPOs, measured as the percentage difference between IPO issue price and closing price on the first trading day:

$$Underpricing_i = \frac{Close\ of\ first\ trading\ day_i}{IPO\ issue\ price_i} - 1 \tag{1}$$

In contrast, we will use the term discount for selldowns and other SEOs, which is measured as the percentage difference between the last trade before the offer and offer price:

$$Discount_i = \frac{Offer\ price_i}{Last\ trade\ before\ offer_i} - 1 \tag{2}$$

For the purpose of this paper, the last trade before offer will be the closing price of the last trading day before the selldown.

1.3 Research design

Following Kumar (2011), we adopt a quantitative, cross-sectional, deductive research approach. We choose a quantitative approach over a qualitative for a number of reasons. First, the nature of the topic lends itself to quantitative analysis. Our research questions will be most thoroughly answered through the analysis of a larger quantity of data given the nature of equity capital markets data. Second, quantitative studies make up the vast majority of research within the ECM literature, and financial economics in general, making comparability less viable for a qualitative study.

The sample we use consists of equity markets transactions available from the Dealogic database. Whilst the data collected is mainly cross-sectional, we use data points before, during and after each selldown occurs. To be a true longitudinal study, we would have to follow each company linearly through time with each occurring selldown. This is not the approach we take, as we do not e.g. evaluate previous selldowns to predict features of future selldowns. We therefore argue that our study is mainly cross-sectional with a longitudinal element.

The process of induction requires the researcher to infer a general relationship between observed factors in the sample. When taking a deductive approach, on the other hand, the researcher will rely on existing research to formulate hypotheses which can then be tested empirically and either be accepted or rejected. As suggested by Kumar (ibid.), a deductive logic is the best fit for a quantitative approach, which the objectives of this paper merits. The deductive logic is particularly useful for this topic given the large body of previous literature on IPOs and, to a lesser extent, on SEOs. Relying on established research is crucial when the topic at hand is complex in its nature. The researcher could not reasonably be expected to induce specific relationships based on observations without relying on previous literature, and would be less likely to correctly identify causality. The deductive approach is a bottom-up process, where existing evidence is gathered and a fundamental understanding of issues is obtained (Bryman and Bell, 2007). Using existing literature as a foundation, one or more hypotheses can be formulated. The following data collection and analysis may also draw upon previous methodological principles and findings. The outcome of the analysis should result in rejection of the null hypothesis or failure to reject the null hypothesis. The final step involves a somewhat inductive element; results are evaluated and potential causes are discussed on basis of the existing literature (Bryman and Bell, 2007).

Based on our three main research questions, we formulate ten specific hypotheses, which can be tested empirically using a dataset of global selldowns. Specifically, we employ various econometric estimation techniques to test the null hypotheses. We compare our results with existing research in an attempt to explain the effects we find.

1.4 Delimitation

This paper investigates selldowns, a relatively unstudied area of the equity capital markets (ECM) literature of an otherwise well-covered field. We choose to limit the scope of this paper in a number of ways, which will be explained in the following:

We study dynamics revolving around selldowns, specifically effects of sponsor-backing on discounts, aftermarket performance and timing. The analysis and effects found are not meant to form the basis of a portfolio- or trading strategy. The scope of this paper does not extend to determining whether the effects we estimate are also likely to be found in the future.

We study only developed countries as defined by MSCI (2017). Results may therefore not be applicable to emerging markets and other less-developed capital markets, as these are likely to display different capital markets dynamics. Further, we exclude micro-cap transactions. This is partly due to poor data quality, but also because micro-cap shares have been shown to exhibit different pricing dynamics than companies defined as small-cap or larger (Konku et al., 2012). Therefore, results are only applicable to companies classified as small-cap and larger.

Because we only include IPOs with subsequent selldowns, implications of this study cannot be used to evaluate ECM transactions in general. Additionally, our research is not applicable to existing general research on seasoned equity offerings. Raising equity capital for the company shares only few features with selldowns, and it is not reasonable to expect results on pricing dynamics to hold for SEOs in general.

We do not take into account operating performance or fundamental valuation metrics. We cannot conclude in general how the operating performance of sponsor-backed IPO companies differs from non-sponsored companies. That is, we only consider impact on company prospects in the eye of investors, as implied by changes to the share price.

Previous studies on long-run performance typically consider a time period of 3-5 years of aftermarket performance. The time horizon over which we consider price performance is limited to one year. Thus, this paper is not perfectly in line with such previous studies on long-run performance.

1.5 Structure of the paper

As part of our deductive approach, section two and three are devoted to an introduction to the private equity model as well as an extensive review of ECM literature. Relying on this literature review, we formulate and explain our hypotheses in section four. Section five and six contains descriptive statistics for the sample and the various methodological choices taken in the estimation process. Here, we devote significant effort to ensure that we take into account various potential statistical issues in our sample and address these in an appropriate manner. Section seven contains the application of various econometric models used for hypothesis testing. We weigh evidence for- and against each hypothesis in this section and discuss these further in section eight. In this section, we go into depth with the implications of our findings and how they should be seen by practitioners. We further give our take on potential drawbacks of data and methodology, which will have implications for future research on the topic. Lastly, section nine contains the overall conclusions for this paper.

2 Equity capital markets and private equity

2.1 The IPO and selldown process

An initial public offering (IPO) describes the process by which a privately held company lists on a public exchange, making their shares available to the wider public. After having made the decision to go public, shareholders in the company will work closely with an investment bank (typically also underwriting the issue) guiding them through the process (Lowry and Schwert, 2004). After a period of marketing the IPO to potential investors, and having assessed the interest the issue receives, the underwriter will start the bookbuilding process (Ramirez, 2011). Bookbuilding is a special auction process in which investors put in orders for a quantity of the shares depending on the price. The investor can put in limited orders at different price points, or unlimited orders. The price may be determined in advance – a fixed price IPO – or indicated as a range – an open price IPO (Alavi et al., 2008). In the case of an open price IPO, the adviser will choose one single offer price at the conclusion of the bookbuilding process, just prior to the start of trading, based on the investor demand. The underwriter will, in most cases, aim to set the price lower than necessary to sell the full issue, which means that the issue is oversubscribed (Carter and Strader, 2002). At the underwriter's discretion, a percentage of each original order will be filled, with favoured investors sometimes receiving a higher percentages of their order than others (Binay et al., 2007). The underwriter is highly incentivised to selling all shares in the offering, as the investment bank will have committed to buying the shares themselves if unsuccessful. In addition to underwriting the issue, the underwriter is obligated to initiate coverage of equity research of the company at the IPO and for some time after. Following the IPO, the shares of a given company are traded in public markets, providing them with a liquid market in which they can raise further capital if needed. When blocks of shares already listed are sold, it is referred to as a seasoned equity offering (SEO) (Gokkaya and Highfield, 2014).

In many cases, the original owners will not sell all their shares at the IPO (Schöber, 2008). As information asymmetry at the time of the IPO is likely to be more severe than after the share has a proven track record, and has had a period of increased scrutiny under the filing requirements of public companies, there is signaling value associated with retaining a significant holding after float. Selling shareholders will typically be barred from selling any more shares for a period of time after the IPO – most often 180 days – in order to ensure that new investors can invest at the IPO and not fear price pressure due to a liquidity event shortly after (Keasler, 2001). It will also ensure that any loss sustained by new investors also hurts selling shareholders.

If the retained holdings of the initial shareholders are considerable the shareholder may choose to sell shares via one or more selldowns, a public market exit strategy that has increased in popularity in recent decades (Bortolotti et al., 2008). Selldowns entail a new bookbuilding process, which, in the case of the accelerated selldown, is typically announced after the market has closed as the announcement would otherwise interfere with the price of the share. For the same reason, bookbuilding and allocation must be completed before the market opens the next morning (Ramirez, 2011).

The main purpose of doing a bookbuilding process, rather than selling in the open market, is to have the smallest possible price impact. General theory of supply and demand would predict a sharp fall in the share price if a significant share of the company is suddenly put up for sale if demand does not change simultaneously. Rather, the seller appoints one or more bookrunners, typically being large banks with many institutional clients, to sell the stake. The idea is that the bookrunner will leverage its relationship with institutional investors, and find those willing to acquire a block of shares, without pushing up the price. The reason that bookbuilding has become the dominant sales process for both IPOs and selldowns is that each investor is forced to reveal its willingness to pay. In aggregate, this gives the bookrunner an estimated demand curve, which allows him to choose the one optimal price considering also the effect on aftermarket trading. In addition, bookrunners and institutional investors have repeated interaction, improving information flows. In the first ever study on the topic, Bortolotti et al. (2008) show the global convergence in method towards accelerated selldowns with an increasing share from 1.1% of all SEOs in 1991 to 36.5% in 2004.

2.2 Private equity

The term private equity (PE) covers an investment company specialised in the ownership of equity, which is not publicly traded. The structure and incentives of the private equity model differs from most other ownership types, and the resulting impact on performance has been widely researched. The first ever private equity fund was raised by Kohlberg Kravis Roberts & Co L.P. in 1976 (Seretakis, 2012) and has since risen in popularity with the past two decades seeing high growth rates in exit value from private equity funds (Bain and Company, 2017). The chart below shows the development in total deal value of private equity exits for each year. In addition to high growth rates, the chart also shows dramatic impact of the 2008 financial crisis and the the 2001 IT bubble.

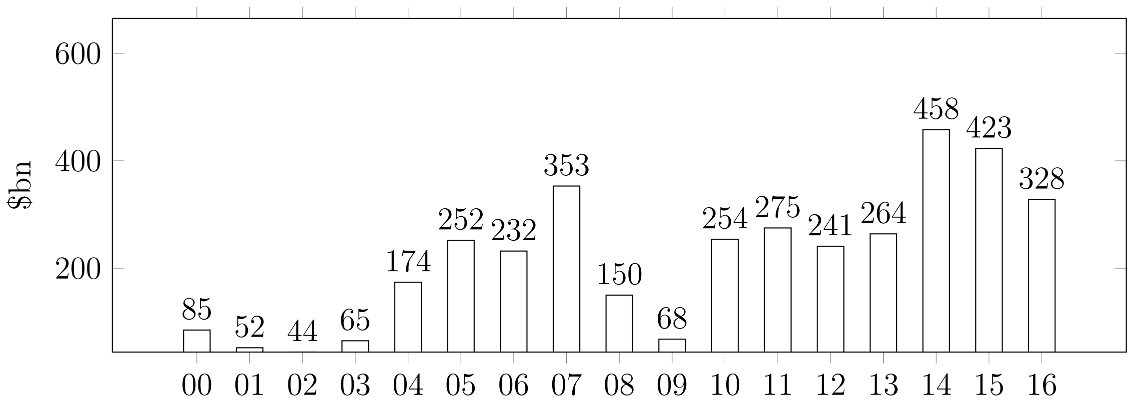

Figure 1: PE-backed buyout value. Source: Bain and Company (2017)

The last 6 years has seen fund-raising by private equity firms increase two-fold, indicating continued and increasing support for the private equity model among investors. Performance generally been found to be higher for private equity-owned companies. Harris et al. (2014) find overperformance of 20-27% over the fund's life or 3% annually. Kaplan and Sensoy (2015) find similar results in their survey but note that overperformance was more consistent before 2000. However, not all research speaks in favour of private equity with e.g. Phalippou and Gottschalg (2009) finding 6% underperformance when adjusting for fees.

2.3 The private equity model

2.3.1 Investment approach

The most common private equity investment type is the leveraged buyout (LBO), an acquisition type where the private equity firm finances the transaction by placing debt in the newly acquired company, using relatively little equity from the fund (Seretakis, 2012). The private equity firm borrows heavily against the assets of the acquired company to stretch the capital in its fund further and leverage returns. The private equity firm will usually focus on majority stake investments, meaning they take over corporate control of the target company. Target companies can be either privately held or listed. Due to acquisitions being largely debt financed, not all companies are equally attractive for private equity firms. The target company should provide stable cash flows in order to be able to service debt, meaning that companies exposed to mature industries are more likely to be targeted by private equity (Rosenbaum et al., 2013). A special sub-type of private equity is venture capital. Venture capital funds (VCs) enter investments at an earlier stage compared to private equity in general. Therefore, VC investments tend to be less levered, and VCs are also more likely to buy minority stakes in portfolio companies (Berk and DeMarzo, 2017).

The median holding period for private equity investments in the period 2004-2016 varied between 3.3 years and 6.1 years (Preqin, 2016). When the private equity firm decides to exit its investment, three main channels exist. The first option is a sale to another private equity fund, a so-called sponsor sale. The second option is a strategic sale to a company in the same, or a related, industry, where the typical rationale is realisation of synergies. The third option is a public market exit via an IPO. The IPO option is costly and time-consuming but will allow access to public capital markets, which means unlocking the liquidity premium as well as a gradual exit with potential for greater returns compared to a private-to-private transaction (Berk and DeMarzo, 2017). IPO exits made up 15-25% of all private equity exits between 2010 and 2016 (Bain and Company, 2017).

2.3.2 Structure and performance metrics

Private equity firms are organised in funds with one or more General Partners (GP) and Limited Partners (LP) (Berk and DeMarzo, 2017). The private equity firm itself takes the role as GP, having responsibility for management of portfolio companies. The partners of the private equity firm will often be required to invest a considerable amount personally into the fund alongside LPs. Additionally, GPs have unlimited liability. LPs are typically institutional investors and wealthy individuals, having no voting rights in portfolio companies and limited liability. Once LPs have committed to investing in the fund, they cannot immediately liquidate their investment before expiry of the fund. They must wait until the private equity firm exits the investments after approximately 3-7 years. The fund will have a predetermined expiry date at which point all investments of the fund must be exited and proceeds returned to GPs and LPs, respectively (Stoff and Braun, 2014).

The GPs typically receive a 2/20 fee, meaning 2% of invested funds in flat fee plus 20% of profits above some predetermined hurdle rate (Najar, 2017; Stoff and Braun, 2014). Owners of the private equity firm are additionally incentivised by their personal investment in the fund. The performance fee is usually calculated as a percentage (most often 20%) of the difference between the compounded value of the hurdle rate and the return on the investment. The GP is not incentivised to produce the highest absolute percentage return, but rather to maximise the Internal Rate of Return (IRR) on each investment. This can result in the optimal strategy being to sell a portfolio company even when the GP expects value to increase further. The IRR-based performance fees and fixed fund expiration of private equity funds causes private equity firms to have different exit timing incentives relative to non-PE owners (Phalippou, 2008). Specifically, private equity firms are more likely to exit investments sooner than other types of investors given their fixed investment horizon and time-based fee structure.

2.3.3 Proposed benefits of the private equity model

We highlight three commonly suggested benefits of private equity investments:

Corporate governance

Private equity investments are generally thought to reduce agency problems by aligning incentives of management and owners. In general, owners want to ensure that managers abstain from slacking, empire-building and excessive risk aversion. After having acquired a company, private equity firms usually seek to install incentive schemes aimed at exit in the foreseeable future, e.g. by awarding management stock options that vest at exit, the value of which depend on the price obtained (Wright et al., 2009). In some cases, the PE firm may replace senior management altogether. As the boards of private equity companies are smaller, they are also thought to be more effective and value-creation oriented (Clarkson et al., 2016; Seretakis, 2012). Additionally, the free-rider problem associated with dispersed ownership is eliminated, leading to more effective monitoring.

Operational expertise and international expansion

Private equity firms seek to improve operating performance in their portfolio companies, and use international experience to broaden its scope to new markets. Some private equity firms are specialised within selected industries, where they have specific experience with turnarounds (Bernstein and Sheen, 2016). In addition, private equity firms often hire management consultants, or make use of in-house management consulting resources (Matthews et al., 2009). General sponsor overperformance has been linked to management expertise present in PE and VC firms, both in terms of operating performance, but also due to extensive experience with capital market transactions (ibid.). Because earnings multiples often form the basis of valuations, decreasing costs leads to higher margins, in turn increasing valuation.

Tax and agency benefits of leverage

The benefits of leverage is twofold: First, through tax deductions of interest expenses, high interest payments form a tax shield decreasing the effective tax rate for the company (Wright et al., 2009). Second, some claim that there are agency benefits of leverage. Jensen (1986) coined the term, which describes how higher leverage disciplines management by creating a pressure to produce cash flows and cut unnecessary costs, discouraging empire-building in the process. Third, conventional finance theory tells us that leverage increases the variability of returns and thus should increase expected returns in a risk-return trade-off setting. In sum, these effects work to increase returns of private equity through the use of debt.

3 Discounts, performance and ownership effects

Due to the extremely limited body of research on selldowns (Bortolotti et al., 2008), the literature review will draw from two related fields: IPO research and general SEO research. IPOs are highly relevant, because the accelerated selldowns that we focus on in this paper occur as a part of an IPO exit process. Therefore, IPO underpricing and long-run performance is relevant to build the analysis on. Likewise, research on SEOs can complement and contrast our topic and prove useful in formulating hypotheses and methodology.

3.1 IPO underpricing and selldown discounts

The term underpricing describes the phenomenon that the price of a newly issued share often increases substantially between issue in the morning and the closing of the first trading day, meaning that a significant amount of money is "left on the table" by selling shareholders at the IPO. Underpricing became a topic within finance research as early as the 1970's, with Stoll and Curley (1970), Logue (1973) and Reilly (1973) being among the first to prove the presence of the phenomenon. There is consensus on the existence of IPO underpricing, but the significance, cause and effects of underpricing are still widely discussed.

The lack of consensus may be due to the constantly changing size of underpricing. In their study of British IPOs, Chambers and Dimson (2009) find that underpricing has increased from less than 4% in the years between the two World Wars to 19% in the period between 1986 and 2007, noting that underpricing increases in high-IPO volume periods. Loughran and Ritter (2002) find similar results for the U.S. market, and is additionally able to identify a spike in underpricing of 65%, occurring during the IT-bubble. In the UK, Levis (1993) finds average first-day returns of 14% between 1980 and 1988.

Discounts in SEOs are smaller than in IPOs, likely reflecting the fact that an IPO is a riskier transaction, the share having no track record, which may result in significant information asymmetries as to the quality of the company. Additionally, there is no established market price on which to base the IPO offer price, making various valuation metrics the only method for deriving the share offer price, and the investor will have no market consensus to contrast his opinion against. Researchers also find varying levels of discount for SEOs, depending on different periods and geographies. Most studies find SEO discount to be significantly smaller than IPO underpricing, ranging between 2.4% (Bowen et al., 2008) to 6.7% (Dempere, 2012). Interestingly, it is hypothesised that while a selldown is issued at a discount relative to the previous closing price, it also tends to have a negative impact on price in the day following the transaction (as measured from closing price the prior day), which may be due to the sudden increase in supply, but could also be influenced by e.g. signaling effects. Kim and Purnanandam (2014) find that the market reaction to equity offerings is in fact negative, with companies with poor governance structure experiencing worse market reactions of offering announcements. The common rationale is that debt is cheaper than equity, why high-quality companies will issue debt rather than equity (Berk and DeMarzo, 2017). Therefore, choosing to raise equity capital typically tells markets that (i) the company is unable to raise capital in debt markets and/or (ii) current shareholders believe the share to be overvalued. In some cases, raising equity may be positive news, e.g. if proceeds are used to finance value-adding projects or to save the company from bankruptcy. Selldowns, on the other hand, are unlikely to send a positive signal. In the absence of other motivation to sell, the implied signal sent by the current owner, having superior information, is that the share is overvalued.

A host of theories seek to explain the reasoning behind underpricing. According to Ritter and Welch (2002) the primary cause for owners to go public is to gain access to a liquid market as a means of cashing out. What then, prompts these owners to deliberately underprice and essentially lose out on a substantial amount of money on the company's first public market transaction? Since the effect is much more dramatic than price fluctuations for seasoned equities and only occurs on the first day of trading, underpricing could stem from the inability or unwillingness of the issuer and underwriters to correctly assess market demand prior to the issue.

Ritter and Welch (ibid.) argue that theories can be classified according to assumptions of either symmetric or asymmetric information, respectively. Theories based on asymmetric information illustrate the "lemon problem", a term coined by Akerlof (1970). Applying this theorem to equity issuance, the logic goes that for an issuer to be willing to issue shares at the average price, the quality of the company must be below average. Investors will be aware of the dilemma, consequently demanding a discount. By offering a discount at the IPO, issuers signal that they are willing to accept an initial loss in expectation of subsequent increase in price, thereby recouping losses at a selldown transaction at a later date. Strategically setting a lower IPO offer price to maximise total proceeds is known as the information momentum theory developed by R. K. Aggarwal et al. (2002). It posits that larger first-day returns generate more coverage, which in turn decreases information asymmetries. Similarly, when a shareholder announces a selldown, it sends a signal to the market that the share is overvalued, prompting potential selldown investors to require a discount to protect them against adverse price movements the following day.

Other theories assume symmetric information, suggesting that underpricing is deliberate and meant to protect issuers and underwriters from legal action in the event of a sharp decline in price post-issue (Hughes and Thakor, 1992). When prices are already high, pushing prices further will allow investors to pursue legal action if the IPO is unsuccessful, which would explain why underpricing increases during bubbles, e.g. 65% during the IT bubble as found by Loughran and Ritter (2002).

Other studies have examined the role that allocation of shares play in IPO underpricing. For instance, Cornelli and Goldreich (2001) and Hanley and Wilhelm (1995) both suggest that underwriters give favoured institutions larger allocations, allowing them the immediate upside of underpricing on the first trading day. This theory suggests that underpricing is advantageous to underwriters and institutional investors but contrary to the interests of the issuer. Unlike sellers and investors in an IPO, underwriters have no capital at stake, resulting in a very different objective function. The underwriter is more likely to promote larger underpricing at the IPO as investors are the repeat customers of the underwriter, and his concern will primarily revolve around his reputation among investors. Additionally, an IPO with no aftermarket momentum is likely to be seen as a failure by pre-existing shareholders as well, decreasing the chance for the underwriter to be chosen as bookrunner for subsequent selldowns. Others suggest that allocating a large proportion of the float to retail investors may also increase underpricing, arguing that retail investors make investment decisions based on beliefs with no grounding in fundamentals and are likely to sell their allocated shares immediately to realise a profit (Dorn, 2009). In some cases management may also encourage underpricing. In their study, X. Liu and Ritter (2010) find that IPOs in which management were allocated shares in the issue (IPO spinning) had first-day returns 23% above comparable IPOs. Loughran and Ritter (2002) propose the “analyst lust hypothesis”, suggesting that issuers are more concerned with hiring an underwriter with an esteemed equity analyst than they are with the risk of mispricing at issue. Other studies support this claim, concluding that underwriters form local oligopolies and that issuers care mainly about non-price dimensions (X. Liu and Ritter, 2011). Additionally, a 2003 study found that 87% of analyst recommendations at the end of the IPO quiet period were “buy” recommendations (Bradley, Jordan, and Ritter, 2003).

The notion that analysts play an important role seems to apply to SEOs as well, with a study finding that more analyst coverage is negatively correlated with discount, citing information asymmetries in poorly-covered SEOs as the main reason for the discount (Bowen et al., 2008). Likewise, the number of lead underwriters have been shown to decrease SEO discount (Huang and D. Zhang, 2011). These findings support the information asymmetry-hypotheses as they find that discount increases when less information is available to investors. This idea is supported by Chemmanur and Jiao (2011) noting that information asymmetry is the main factor explaining SEO discount. They also find that the higher the share of institutional ownership, the less information asymmetry and lower discount.

Intintoli and Kahle (2010) report that SEO discount has increased over time, from 1.15% in the 1980’s to 2.92% in the 1990’s. They find two potential drivers for discounts in SEOs: First, discount is greater when the float is smaller, supporting the downward-sloping demand curve theory. Second, discount is smaller, the higher the proportion of secondary shares on offer. They hypothesise that owners are likely to pressure underwriters to obtain a higher price, but one might argue that the signaling value associated with offering primary and secondary shares, respectively, is different. As very limited research on selldowns exists it is difficult to say whether discounts differ for selldowns and other SEOs. However, the finding that discount is related to the proportion of secondary shares offered does imply different dynamics when considering selldowns, which consist only of secondary shares. Intintoli, Jategaonkar, et al. (2014) find that SEOs within a year of the IPO, that are more likely to be selldowns, have smaller discounts than other SEOs, which in paper is explained by higher investor demand due to the relatively recent marketing of the IPO. Supporting this argument, Kim and Purnanandam (2014) find that the share price reacts negatively to announcement of an SEO, but only when the proceeds are to be used for funding capital investments, particularly acquisitions.

3.1.1 Motivation behind the timing of selldowns

It is important to make a distinction between different types of SEOs. Selldowns differ from other SEOs as it is a transaction between owners and other investors and proceeds do not flow to the company itself. Nearly all academic literature focuses on SEOs that are at least in part capital-raising, which is likely to lead to widely differing conclusions relative to the topic of this paper: bookbuilt, underwritten overnight transactions of blocks of secondary shares sold following an IPO.

The definition of selldowns used throughout this paper implies that the primary motive of a selldown is monetisation of an equity stake. We consider different types of owners with different optimisation problems. Intuitively, sponsors are punished harder by the passing of time through compounding of the hurdle rate while non-sponsors do not face this mechanism. Their timing decision is subject mostly to the absolute return they can generate with lesser punishment from their cost of capital. Previous research has looked into timing decisions for the two major types of non-sponsors that typically take companies public: families and corporates (through corporate carve-outs). Klasa (2007) find evidence that companies in which family owners sell blocks of shares in the aftermarket have seen larger increase in industry and company market-to-book ratio relative to a sample of matched companies. The paper also finds evidence that selldown companies performed significantly better prior to the selldown in terms of unadjusted returns as compared to the control group, while the effect is statistically insignificant for adjusted returns (but still has a sizeable effect). This indicates that family owners do indeed seek to time selldowns when valuations are high. Similar dynamics have been found for corporate carve-outs. Powers (2003) shows that companies that are carved-out generally have above industry average operating performance metrics at the time of the IPO. These above average metrics then tend to return to the mean following the carve-out, indicating that corporates, too, time the sale of equity at times when performance, and thus valuation, is high. The long-run share price performance also tends to turn negative within a 5-year time horizon following the IPO. Although the paper studies IPOs, we would expect the same dynamics to be in place when corporates sell shares in later selldowns. In sum, evidence from prior studies have shown strong evidence for varying degrees of the timing hypothesis for non-sponsors, which states that equity sales are timed when market valuations are high.

3.2 Aftermarket performance

A related research topic is concerned with the aftermarket performance of IPO firms. Underpricing essentially deals with price fluctuations happening on the first day of trading, while the aftermarket perspective is concerned with the performance of the issue over a period of several years. Underpricing depends mainly on characteristics revolving around the IPO process itself. In contrast, long-run share price developments also depends on continuing performance of the company as well as exogenous factors.

Ritter (1991) uses a sample of 1,500 U.S. IPOs taking place between 1975 and 1984. In his paper, Ritter documented that IPO firms underperformed comparable equity investments by 17% when using a buy-and-hold strategy and holding the issued share for 3 years. He finds that aftermarket underperformance is more severe when the IPO takes place during a high volume period or "window of opportunity" (which also increased underpricing according to Loughran and Ritter (2002)). Additionally, his study also suggests that these windows of opportunity are likely to imbue investors with overly optimistic expectations of growth, especially in younger companies, further increasing underperformance for such companies. This result is similar to previous findings on U.S. markets of 13% underperformance when buying at the closing price of the first trading day and holding until day 250, while supporting the notion that temporary infatuation with new issues are the leading cause (R. Aggarwal and Rivoli, 1990)). Levis (1993) is able to replicate the results for the UK IPO market, measuring between 11 and 23% aftermarket underperformance at the 3-year anniversary depending on methodology, using a sample of 712 IPOs between 1980 and 1988. Results of -47%, -20% and -24% have been found for Brazil, Mexico and Chile, respectively (R. Aggarwal, Leal, et al., 1993), -29% for France (Boissin and Sentis, 2014), 2012), -27% for Spain and -33% for Germany (Jaskiewicz et al., 2005). Generally, there seems to be consensus on IPO underperformance in the long term, as summarised in Jaskiewicz et al. (ibid.), citing 25 studies, 23 of which finding negative abnormal aftermarket returns.

Some theories consider the actions of the company following the IPO a primary determinant of long-term aftermarket performance. A recent study of U.S. IPOs found that M&A activity would significantly adversely affect returns, but only in the case of two or more acquisitions undertaken in the post-IPO years (Amor and Kooli, 2016). Additionally, frequent acquirers are less likely to survive the five-year period following the IPO. These findings are much more dramatic than for companies with more distant IPOs, finding underperformance for frequent acquirers in the sub-2% range (Ismail, 2008). Another study found that initiating dividends within five years of the IPO would be associated with an increase in returns which cannot be explained by the cash flows generated, and must therefore be at least in part due to the signaling effect of dividend initiation (How et al., 2011). These theories suggest that IPO companies are more susceptible to significant reactions to corporate decisions compared to non-IPO companies. However, most companies do not go on acquisition sprees following their IPO, and the dividend effect is not pronounced enough to explain the consistent average underperformance of IPOs.

Another set of theories focuses on execution during the IPO process, sharing features with theories explaining the underpricing problem. Aftermarket performance, like discounts and underpricing, seems to vary over time according to the IPO volume. The idea is that higher-quality firms are willing to go public during low-volume periods, while an open "IPO window" will allow low-quality firms to go public due to greater investor confidence in general. Numerous researchers are able to prove that an IPO is more likely to underperform in the aftermarket the later in the IPO window it is issued (Ritter, 1991; Schultz, 2003). Some evidence suggests that IPOs with prestigious underwriters tend to have less-negative long-run abnormal performance and less underpricing compared to IPOs with less-prestigious underwriters (Carter, Dark, et al., 1998; Dong et al., 2011). This is likely due to a certification effect, whereby investors place more trust in well-established underwriters than less known ones. Another explanation is that reputable underwriters are better at mitigating information asymmetries, because they have more skilled analysts and efficient processes. Other research supports the idea that the approach employed for the marketing of the offering may be related to both underpricing and long-run aftermarket performance. For instance, when more conservative language is used in prospectus and other marketing material, the underpricing tends to be greater and long-run aftermarket performance worse (Ferris et al., 2012). This indicates that underwriters rely on their reputation, making them unwilling to overstate the true value in the prospectus. This is in line with the explanation to why underpricing increases when valuations are generally high (Hughes and Thakor, 1992).

Much less research has been conducted on long-run aftermarket performance in relation to seasoned equity offerings, and the research that does exist is at least partially contradictory. Loughran and Ritter (1997) is one of the few papers dealing with the topic. They find that SEO firms tend to be high-growth companies, which have excellent operating performance in the year leading up to the SEO. This is followed by five years of drastic underperformance immediately after the SEO, which is not evident in price multiples of the share at the time of the transaction. They speculate that firms “manage” earnings to ensure the highest possible share price at the time of the SEO, leaving investors disappointed in the subsequent period. Conversely,Bayless and Jay (2011) find that SEOs is preceded by two years of poor operating performance, and also find that post-SEO performance is worse than for non-issuing peers. The model developed by Chemmanur and Jiao (2011) predicts that when institutions are net-buyers before the SEO, the long-term operating performance seems to improve following the transaction.

In their review of the SEO literature, Bortolotti et al. (2008) find average SEO first-day returns to be between 2.6% and 2.9%. Investigating the drivers of SEO first-day returns, Hull et al. (2012) present four factors associated with higher first-day returns of SEO with 3-day and 21-day announcement periods, which differs from the sample in this paper, having no trading days between announcement and pricing. One of the findings is that lower discounts are associated with higher first-day returns, which could imply that share price moves towards the offer price; shares sold very cheaply will have a large negative impact on trading and vice versa.

3.3 Private equity ownership and ECM transactions

A large topic within the IPO literature, and to a lesser extent SEO literature, is transactions involving companies that are owned, fully or in part, by private equity (PE) or venture capital (VC). The main difference between the two is the stage at which the sponsor enters the investment. Where VCs tend to invest in firms in the early stage of its life cycle, PE firms target more mature companies. With regards to fee structure and the fixed investment horizon, the two are very much alike. VCs face the same time pressure and incentive schemes, making their ownership behaviour in relation to selldowns similar to that of PEs (Berk and DeMarzo, 2017). For the purpose of this paper, we will not differentiate between the two, but consider a company as either sponsor-backed or non-sponsored.

Generally, sponsor-backed IPOs are found to have less underpricing and better aftermarket performance compared to non-sponsored IPOs (Bruton et al., 2010; Hsu and Chang, 2008; Levis, 2011; Meles, 2011; Ritter, 2015). Ritter (2015) finds 3-year 25.2% overperformance of growth-capital backed IPOs. Levis (2011) finds approximately half the underpricing and more than 9% annual overperformance over 36 months post-IPO. There are two main schools of thoughts explaining long-run overperformance of sponsor-backed IPOs: those revolving around certification and those highlighting the agency benefits associated with sponsor-backed companies. Certification may explain why sponsor-backed IPOs consistently have stronger performance than non-sponsored IPOs (Megginson and Weiss, 1991). When a sponsor prices an IPO, he needs not only to consider how to maximize the value of the IPO and following selldowns of the company at hand; he must also factor in potential negative impacts to his reputation, which would decrease the attractiveness of the next IPO the sponsor chooses to market. Conversely, in many non-sponsored IPOs, the selling shareholder is likely to be in the IPO market only once, with the seller typically being a corporate- or family-owner. This means that there are little or no reputational concerns. Additionally, sponsors, almost exclusively closed-end funds, will have to exit their investment in the IPO completely within a foreseeable horizon after the IPO. If the sponsor were to vastly overinflate expectations of earnings potential at the time of the IPO, they would likely suffer from a decreasing share price at subsequent selldowns as a consequence of investors' disappointment, again prompting sponsors to more accurately relay information to the market so as to better manage expectations for future performance. Investors are aware of these effects, which means that information and forecasts by sponsors are considered more credible than those of other types of selling shareholders. Hopkins and Ross (2013) offer empirical support for the firm certification view, finding that the perceived quality of the private equity firm is the primary driver of firm certification, with the post-IPO stake retained having second-highest explanatory power.

As one of the most influential academics on the topic, Jensen (1986, 1989) has argued that private equity ownership in general has numerous agency benefits, including monitoring, expertise and the agency effects of leverage. This has been supported by countless others. There are two arguments why benefits of sponsor ownership may be sustained beyond the IPO. First, effects of managerial practices, strategy and investments made while under sponsor ownership will not disappear immediately. Second, as it is rarely the case that sponsors exit completely at IPO, a large stake will be retained at least until lock-up expiry, meaning that the sponsor has a continued financial interest in the performance of the company.

3.4 Lock-up provisions

Lock-up provisions have become a feature of nearly all initial public offerings (Mohan and Chen, 2002). A lock-up provision is usually agreed upon between underwriters and selling shareholders, where the latter agree to a predefined period in which they cannot sell shares, unless an agreement is reached with the underwriters to waive the provision. It is put in place in part as a signal of confidence on the part of the selling shareholders and insiders, but also to protect new investors against liquidity events in the first months of trading. In a sense, they are catalysts of the certification effect, keeping selling shareholders exposed to the share for longer. As there is relatively little information about the firm available to the public prior to the IPO, investors demand a certain level of commitment from selling shareholders as they possess superior information. The median lock-up length for selling shareholders is 180 days, with IPOs with higher perceived risk having longer lock-up (Goergen et al., 2004). Controlling for company riskiness characteristics, Ahmad and Jelic (2014) prove that longer lock-up periods significantly increase company survivability after the IPO.

Cline, Fu, and Tang (2015) find that the presence and duration of lock-up provisions in an SEO has a positive impact on announcement day returns, but do not find any influence on long-term performance. This suggests that lock-up provisions have similar effects both at IPO and selldowns, namely to assure investors that no more liquidity events are likely in the short-term.

Not all parties have the same lock-up, as e.g. directors with significant holdings often have lock-up periods of more than 180 days. This is more likely to be meant as performance incentive and signaling of commitment rather than protection against liquidity events. In their paper, testing a wide range of hypotheses related to lock-up provisions, Goergen et al. (2004) hypothesise that lock-up and underpricing are substitutes for each other, which implies that, for each price, issuers can choose larger underpricing in exchange for a shorter lock-up period and vice versa. Further, they show that the certification effect allows VCs to have shorter lock-up periods than non-VC selling shareholders.

If laws of supply and demand hold, we would expect decreasing prices as the lock-up expiry approaches due to investors anticipating the coming oversupply of shares, unless markets can be assured that the owners do not intend to sell the locked-up shares. Keasler (2001) proves that this is indeed the case, showing negative abnormal returns in the period leading up to lock-up expiry due to unrestricted shareholders diminishing their positions. Additionally, he finds that the effect is more pronounced when the price has increased since the IPO, speculating that this effect may in part explain why IPOs do, in general, perform worse on a 1-3 year horizon than similar companies that have been listed for longer. However, this is widely contested, with Cline, Fu, Tang, and Wiley (2012) finding no robust relationship and Chong and Z. Liu (2016) finding support for the hypothesis only when using a very short time horizon.

3.5 Share overhang and the clean-up trade

A topic scarcely covered by literature, share overhang refers to the proportion of retained ownership after the IPO. An extensive body of research suggests that pre-IPO shareholders strategically set the IPO offer price low to maximise total proceeds. Then, it is probable that the magnitude of the underpricing and share overhang should have some relationship, i.e. if nearly all shares are sold in the IPO, we would expect small underpricing and vice versa. This is exactly what Bradley and Jordan (2002) find, citing overhang as one of the strongest ex-ante underpricing predictors. Using a sample of nearly 5,000 IPOs, Dolvin and Jordan (2008) find similar results. Further, they conclude that the total cost of going public remains stable over time, despite the very volatile nature of underpricing. This is precisely because of the positive relationship between overhang and underpricing almost perfectly offsetting each other on average.

There is no reason to expect this rationale not to hold for selldowns as well, i.e. the post-selldown stake should also be positively related to the selldown discount. Despite having no specific empirical support, we would expect underpricing to be smaller when the post-selldown stake of the seller is zero. In this "clean-up trade" the seller will no longer care about momentum in the share, as this is the last chance to extract wealth from shares in the company. We would therefore expect the discount in a clean-up trade selldown to be smaller than for all other selldowns. In addition to the prospect theory-inspired argument above, we have reasons to expect why investors would accept a smaller discount. In general, it is reasonable to expect pre-existing IPO shareholders to want to exit their investment completely in the years following the IPO. As long as the seller retains a share, there is a constant threat of liquidation of the remaining holdings, which might put downward pressure on the price. The downward pressure on price is caused by the sudden surplus supply of shares which, if the downward-sloping demand curve holds, should decrease price. Additionally, some investors may sell the shares they buy in the selldown immediately after the offering to lock in the profit from the discount they typically receive. Other investors will be aware of this threat, which may dampen returns as long as the overhang exists. After the clean-up trade, when the last selldown is conducted, the share overhang is eliminated and no concrete threats of liquidity event remain, which should have a positive effect on price.

4 Hypotheses

Despite the large aggregate value of selldowns, very little research exists on market dynamics and effects of financial sponsor ownership in selldowns. We form ten hypotheses based on the review of previous research and agent interplay in equity offerings. The hypotheses we base our analysis on have been divided into three groups: (1) ownership effects, (2) clean-up trade effects and (3) lock-up provision effects.

As previous literature has focused on IPOs and SEOs where capital is raised mainly for the company, and not the owners, we find it valuable and relevant to establish whether the effects of sponsor-backing on discount and aftermarket performance also apply to selldowns. Further, some of these relationships will form the basis for the hypotheses in sections (2) and (3), rendering it necessary to validate the underlying assumptions.

4.1 The effect of sponsor-backing on discounts and returns

The first hypotheses relates to discounts in selldowns. Sponsor-backed IPOs and SEOs have been shown to be issued at less underpricing and smaller discounts, respectively. There is strong evidence that this can be attributed to the certification effect, as sponsors are recurring actors in capital markets (Hopkins and Ross, 2013; Megginson and Weiss, 1991). In this regard, selldowns are no different from IPOs and SEOs with the given sponsor's reputation being at stake when selling shares to investors. Thus, we expect sponsor-backed selldowns to have smaller discounts than non-sponsor-backed ones, holding other factors constant.

H1a: Sponsor-backed selldowns have smaller discounts than non-sponsor-backed selldowns

Previous research has found a strong effect of sponsor-backing on long-run abnormal returns in recently listed companies (Levis, 2011; Megginson and Weiss, 1991; Ritter, 2015). Most selldowns happen within the period where positive abnormal performance has been observed (82% of selldowns in our sample happen within 5 years of the IPO). As we have no theoretical or empirical evidence to the contrary, we expect the selldowns in our sample to follow a similar pattern. The following hypothesis serves as a check to see if other effects around the selldown introduces noise into this previously found relationship. The hypothesis is as follows:

H1b: Sponsor-backed selldowns have greater 1-year abnormal returns as

compared to non-sponsored selldowns

Taken together, hypothesis 1a and 1b are included to test whether two fundamental relationships that have been found in previous research on IPOs and SEOs, are also applicable to selldowns. Confirming these two hypotheses would be a significant contribution to current gaps in the literature when it comes to selldowns. It allows us to isolate ownership type effects on share price in the short- and long-run in companies that do selldowns. Further, it helps us in interpreting the results in the following hypotheses that apply more specifically to the special case of selldowns.

4.2 Clean-up trades

No previous research has focused specifically on clean-up trades. Therefore, we rely on literature relating to share overhang, and draw upon conventional finance theory. Using supply and demand theory, the information momentum effect and prospect theory, we expect that discount will be smaller for clean-up trades for three reasons. On the supply side, when the share overhang is large, rational investors will factor in these potential supply shocks when determining the share price. This implies that when investors buy shares in a clean-up trade, they will be fairly certain that no further supply shocks will impact the price in the near term. This should effectively remove the discount that relates to the share overhang. On the demand side, a clean-up trade also implies that investors with the intention of increasing their exposure to the given company, without causing a significant price impact, will have their last chance of doing so. This could potentially increase demand for shares in the selldown if investors are competing for shares, in turn increasing demand. With respect to the seller, when the share overhang is eliminated, the selling shareholder is more likely to seek to minimise discount, as the arguments for larger discounts in both the information momentum effect as well as prospect theory no longer hold. Thus, we hypothesise that clean-up trades will have significantly smaller discounts than other selldowns:

H2a: Discounts are smaller for clean-up trades than other sponsor-backed selldowns

signaling effects aside, the reason for expecting a negative reaction to the announcement of a selldown is the price equilibrium effect of a supply shock when a large block is put up for sale, which rationally should decrease the share price in the short term. Generally, the announcement of a selldown will be bad news for existing shareholders that do not partake in the selldown. However, this effect should be weaker when it comes to clean-up trades. Despite the temporary increase in supply, market participants know that there are no further large blocks from pre-IPO investors to be put in the market. This should make the negative price impact less severe, by instilling confidence in investors that there will be no more liquidity events in the foreseeable future. To our knowledge, no empirical research has been conducted on the topic but the evidence of share overhang in SEOs suggests that the share overhang increases discounts and decreases first-day returns (Bradley and Jordan, 2002; Dolvin and Jordan, 2008). Taking this notion one step further, we anticipate first-day returns for clean-up trades to be less negative:

H2b: First-day returns are less negative after clean-up trades than other sponsor-backed selldowns

In extension of previously found effects between sponsor-backing and positive abnormal returns for recently listed companies, we hypothesise that when a sponsor exits a company, this effect will disappear over time. That is, when a sponsor sells the last remaining stake in the company via a selldown it will reduce, and potentially eliminate altogether, the positive abnormal returns. The logic behind this argument revolves around the agency-cost minimising effects of PE ownership (Clarkson et al., 2016; Seretakis, 2012). When sponsors are no longer involved in the operations of the company, we would expect positive abnormal returns to revert over a 1-year time horizon. The hypothesis is formulated as follows:

H2c: Clean-up trades exhibit no 1-year abnormal returns after the selldown

4.3 Lock-up provision effects

The following five hypotheses relate to shareholder behaviour around expiry of the lock-up provision. These serve to uncover whether significant differences in behaviour between sponsors and non-sponsors exist when it comes to timing of selldowns in relation to lock-up expiry. In addition, we analyse potential effects on timing from share price performance since the IPO, and how this timing impacts market expectations. Sponsors are balancing time and absolute return to maximize IRR (Phalippou, 2008), putting them under a time pressure to sell shares not offered at the IPO. Thus, we would expect that they do selldowns shortly after lock-up expiry, while non-sponsors do not face the same pressure, and are more likely to time selldowns when valuations are high. This implies that sponsors, acting rationally to the incentive scheme they face, will do selldowns at a shorter time from lock-up expiry than non-sponsors, holding other factors constant. The sample data seems to indicate that sponsors, on average, sell shares quicker and with less focus on valuation. The average (median) sponsor-backed selldown happens 872 (615) days from the IPO where the market capitalisation has increased by 77% (46%). In comparison, non-sponsored selldowns happen 1,331 (910) days from the IPO where the market capitalisation is 97% (55%) higher. This supports the argument that sponsors sell quicker with less focus on valuation in absolute terms. We test the following hypothesis to see whether that is in fact the case for our sample of selldowns:

H3a: Sponsors are more likely to do selldowns sooner after lock-up expiry than non-sponsors

Following the reasoning in H3a, we expect non-sponsors to put relatively more weight on valuation than time compared to sponsors. Sponsors are optimising selldown timing based on a function of time and absolute return, with significant negative impact of time on the performance-based part of a standard private equity fee structure. In comparison, non-sponsors are not directly under the same measurable time pressure, and will have more discretion in selecting the right time to do a selldown. Thus, we would expect their timing decision to be correlated with market valuation. Our hypothesis is that non-sponsors will be more inclined to sell shortly after lock-up expiry if the share has performed well since the IPO. The hypothesis can be stated as follows:

H3b: The likelihood of non-sponsors doing a selldown shortly after lock-up expiry is positively related to increase in valuation since the IPO

A natural extension of the reasoning behind H3b would be that if markets are aware that when a non-sponsor does a selldown shortly after lock-up expiry it is a signal that the company might be overvalued, we would expect a negative market reaction following the announcement. Rational investors would see this as a sign of the valuation of the company being high relative to the intrinsic value as assessed by insiders. In other words, the seller has insider information that the outside investors do not, and will be better equipped to determine the fair value of the company. When the insider chooses to sell, it is a sign that the fair value is high relative to the market value. Thus, we would expect to see a negative effect on discount and/or first day returns as investors price in the signaling value implicit in the selldown timing. We test both discount and first-day returns as the price effect could be through either one, depending on how fast markets react to the signaling value of the transaction. We formulate the hypothesis as follows:

H3c: The short-run market reaction in terms of discount and first-day returns will be relatively more negative when a non-sponsor does a sell-down shortly after lock-up expiry as compared to other non-sponsored selldowns

In extension of H3b, we hypothesise that this relationship does not hold for sponsors. Again, this hypothesis is based on the timing pressure sponsors are subjected to. The reasoning goes that sponsors will want to do selldowns shortly after lock-up expiry, regardless of the price increase it has seen since the IPO. They will not have the same leeway in waiting out for further value gains. That implies that the increase in share price from the IPO will not have the same effect on the timing decision of selldowns for sponsors. This makes for the following hypothesis:

H3d: The likelihood of sponsors doing a selldown shortly after lock-up expiry is independent of the increase in market valuation since the IPO

Following this, and analogous to H3c, we do not expect to see a market reaction that deviate significantly from other sponsor-backed selldowns. In other words, we expect markets to react in the same way to a sponsor-backed selldown that takes place shortly after lock-up expiry as it does to other sponsor-backed selldowns. We conclude this section on lock-up provisions with the following hypothesis:

H3e: The short-run market reaction in terms of discount and first-day returns will not be significantly different when a sponsor does a sell-down shortly after lock-up expiry as compared to other sponsor-backed

selldowns

These hypotheses aim at making concrete predictions for when markets can expect a given type of owner to do a selldown. Further, H3b and H3d seek to give an indication of what the timing of a selldown for different owner types signals about the current valuation of a company. H3c and H3e test if this potential signal is evident from short-run market reactions. We extend the knowledge obtained in previous research regarding intrinsic motives and incentive structures to gain a better understanding of owner behaviour when it comes to the timing of selldowns. Understanding why owners might do selldowns at a given time also helps in understanding the state of the company and ownership composition at that given time.

5 Empirical research approach

5.1 Data collection

5.1.1 Geographies

We take a global perspective throughout the analysis. However, some limitations do apply to countries we choose to include in our sample. There is substantial evidence that emerging and developing markets are influenced by factors like weak corporate governance and insufficient legal institutions (see e.g. Sanyal et al. (2014), Fernandes and M. A. Ferreira (2009), Griffin et al. (2010)). The efficiency of stock markets in emerging and developing markets seems to be questionable when it comes to equity issuance. In order to limit the impact of these additional risk factors, and due to limited liquidity and PE penetration, all emerging and developing markets are excluded from the analysis (Israel is excluded as no selldowns in the country were identified). When making the distinction between developed and non-developed markets, we use the classification of MSCI's world index (MSCI, 2017). For the sample data, we define country as the nationality of the exchange on which the firm chooses to list. If the firm is dual-listed, the firm is allocated to the deal nationality as defined by Dealogic, which is the country to which the firm is exposed the most.

5.1.2 Equal-weighted returns vs. value-weighted returns

There is no apparent consensus in the body of previous literature when it comes to weighting of sample observations. Some researchers have used equal-weighted returns while others have used value-weighted returns. In his seminal study, Ritter (1991) relies on equal-weighted returns to determine long-run IPO returns. This is a widely adopted approach across the ECM literature (Jaskiewicz et al., 2005). Taking a more portfolio-oriented approach to returns calculations, Dong et al. (2011) and Levis (2011) find that both equal- and value-weighted returns yield similar and significant results in their respective studies. Yet other papers find that one yields statistically significant results, while the other does not, with no consensus as to which one is more appropriate (Ritter, 2015; Ritter and Welch, 2002). Fama (1998)

Table 1: Geographies included in sample as classified by MSCI

Europe	**Americas**	**Asia-Pacific**
Austria	Canada	Australia
Belgium	United States	Hong Kong
Denmark		Japan
Finland		New Zealand
France		Singapore
Germany		
Ireland		
Italy		
Netherlands		
Norway		
Portugal		
Spain		
Sweden		
Switzerland		
United Kingdom		

favours the value-weighting approach, and argues that bad-model problems become more severe using an equal-weighting approach as more weight is allocated to smaller companies with problematic return patterns. The applicability of this argument is limited in this paper, as our sample excludes smaller companies with significant liquidity risk. Further, the impact of company size on returns and discount is controlled for in the specified models through the inclusion of a market capitalisation control variable in our models.

Value weighting returns is the most intuitive way of interpreting results in a portfolio perspective, where returns to a given investor is considered. However, this paper deals with market dynamics around selldowns, as well as aftermarket stock price behaviour, rather than performance over a multi-year period. That is, we are interested in general effects derived from ownership and signaling rather than setting up the basis for a trading strategy. We use equal-weighted returns throughout this paper as we see all selldowns as being equally important for deriving general effects. In addition, we exclude small companies and control for size effects in the econometric models, in turn limiting the effects that has been argued to be problematic in an equal-weighting approach.

5.1.3 Exclusions

Starting with a gross sample of 63,942 transactions by 35,126 companies, we exclude observations based on various criteria in order to identify a sample consisting only of relevant selldowns. The gross sample consists of all equity market transactions in the identified countries for the sample period. We exclude transactions based on deal value of the issue and the market capitalisation of the firm at the time of issue. All IPOs with a deal value of less than EUR 50m and selldowns with a deal value of less than EUR 5m are excluded. Further, companies with a market capitalisation of less than EUR 100m at the time of IPO are excluded. The cut-off points are set to free our sample of smaller firms that would introduce a range of issues in the models. The market efficiency of small-cap shares has previously been shown to be weaker than shares of larger firms, and could potentially skew our sample in a number of ways (Loughran and Ritter, 2000). The cutoff point for selldown deal value has been set to include only selldowns that follow the bookbuilding process in which we are interested.

We include only firms that go public and perform subsequent selldowns in the sample period. In order to measure aftermarket performance, we require one year of trading following the transaction. Thus, we eliminate all transactions after February 16, 2016. Identifying selldowns correctly is important to the reliability of the results. Funds raised for the company rather than for the owners will introduce noise and potential bias into the estimates. If funds are raised for the firm, markets will react in part to the signaling value from the pecking order theory (Berk and DeMarzo, 2017) and proposed use of funds (Silva and Bilinski, 2015). Thus, we eliminate all transactions where issuer (the company itself) sells existing shares or issues new ones, allowing for a thin margin where e.g. a small amount of treasury shares are included in the offering. In addition, we require the transaction to take place while markets are closed for the discount and first-day returns to contain information from the selldown announcement. We use announcement and pricing dates to exclude selldowns that are not in the form of an overnight accelerated bookbuilding. If we were to have transactions in our sample where the announcement and pricing dates were more than a day apart, the share price reaction would rationally be incorporated immediately on the announcement day, and the measures of discount and first-day returns would not capture the actual impact. Using these two exclusion criteria effectively ensures that we are left only with actual selldowns where the discount and first-day returns will capture the market reaction of interest.

For the specific companies left in the sample, we exclude close- and open-ended funds, real estate investment trusts and other funds. Redemption and issuance fees could potentially introduce noise into discount measures, and pricing behaviour deviates from frequently traded equities (Russel, 2005), meaning that information will not as efficiently be incorporated into returns immediately after a transaction.

5.1.4 Benchmarks

Correctly adjusting raw returns to reflect only abnormal returns is essential in capturing true outperformance. Misspecifications will compound over time, making correct market adjustment more important as the time horizon increases. Longer-term aftermarket abnormal returns are highly sensitive to "normal" returns adjustments. As Fama (1998) points out in his paper, testing abnormal returns are in effect a joint test with the model of expected normal returns employed. In turn, our results will contain noise from this bad-model problem, as all known models of expected returns are incomplete. If we are estimating biased expected returns, this will compound over time and undermine the reliability of the results. One potential bias could arise from the clustering of IPOs and selldowns when equity market valuations are high and increasing, implying that a larger part of aftermarket returns is likely to come from general market increases rather than abnormal returns.

Ideally, to fully capture abnormal returns of a given company, we would compare each selldown firm with an identical firm that does not do a selldown at the time. However, as this is not feasible, we use sector- and country indexes as a proxy for normal returns. Other factors like firm size and book-to-market ratio have been shown to have an effect on expected returns (Fama and French, 1993), meaning that our model of expected normal returns, too, is incomplete. However, we further reduce the impact from the bad-model problem through the inclusion of control variables and a time horizon that does not exceed one year.

We use MSCI's World sector indexes as the best available proxy for normal returns. Unlike the ACWI indexes, the World indexes include only developed economies, matching the exchange nationalities in the sample data. Further, sector indexes will capture sector-specific valuation fluctuations that country- or broader market indexes cannot. For instance, a country index would not capture the high valuations of IT sector companies in the early 2000's, nor the low valuations of energy companies in 2015-2016 in what was otherwise record-high market in many developed economies. Further, we argue that by excluding very small companies, the transactions in the sample are more likely than not to have a global investor base and operating exposure. This should make the companies more correlated with international equity markets within the sector than the country index. For robustness, we also report abnormal returns adjusted with country indexes for the country in which the company is listed. Country indexes can capture potential local effects that sector indexes cannot. For this, we rely on MSCI's country indexes.

Using index adjustment assumes that a company has a beta of 1 with respect to the index it is allocated to. In previous research, it has been argued that newly listed companies have beta coefficients much larger than 1, making this adjustment approach inappropriate (Barber and Lyon, 1997; Leleux, 1993). However, as the selldowns in our sample take place 3 years from the IPO on average, they have a significant trading record, making them more comparable to other companies included in the indexes. Another critique is the "rebalancing" bias (Brav, 2000) which arises from the implicit rebalancing of a value-weighted benchmark portfolio when sample firm returns are not rebalanced in a similar manner. This is indeed the case in the sample as we use value-weighted indexes, but the shorter time horizon considered should make this issue negligible. Further, using both sector and country indexes allows for a greater certainty that the results are not driven by misspecification of the benchmark portfolio.

Additional return adjustments were considered but deemed infeasible due to data-, theoretical- or empirical limitations. This includes the commonly used matching portfolio approach, where the sample firm is matched with a listed company, typically based on size and/or industry. Brav and Gompers (1997) argue that matching with individual companies introduces noise and firm-specific risk into the model, and instead champions matching to industry portfolios like the approach used in this paper. Measuring abnormal returns in relation to the CAPM was also considered, but due to vast evidence of a weak relation between beta and returns (Bornholt, 2013), we choose not to employ this method. Adjusting with a multiple factor model, like the Fama-French model (Fama and French, 1993), has also been used in previous literature (see e.g.Brav and Gompers (1997), Chan et al. (2008), Cline, Fu, Tang, and Wiley (2012), Krishnan et al. (2011)). Although this would serve as an efficient further robustness check, we do not have sufficient data to make this return adjustment, leaving us with sector and country index adjustments.

5.1.5 Lock-up provisions

Using data from the Dealogic database, some manual corrections are required to correctly define when a given shareholder's lock-up expires. For transactions where different selling shareholders have different lock-up periods, the provisions follow each shareholder. In other words, we track and match selldowns depending on the selling shareholder(s) and use the relevant lock-up period for determining how far from expiry the selldown is undertaken. Lock-up periods are often waived in the sample, meaning that the underwriters allow a transaction by the locked-up entity prior to the expiry. In that case, the time from lock-up release is set to zero days. Although Dealogic offers data on whether the lock-up provision was waived or not, some selldowns were undertaken prior to expiry of the provision without Dealogic identifying a waive. In these cases, where no evidence to the contrary was found, we assume that the provision was in fact waived and set the selldown to zero days from expiry.

5.1.6 Clean-up

Identifying a clean-up trade involves some judgment. The effects we want to estimate occurs when no further block trades are likely to happen in the foreseeable future. That is, when the overhang of shares retained by the owners at the time of the IPO is effectively eliminated. This would require identifying ownership stakes for all relevant shareholders at the time of each transaction, while further defining which stakes are likely to come up for sale, and which are held long-term. As data is not readily available to apply this method, we define clean-up as when the last pre-IPO sponsor sells the remaining shares in the company. That is, when the aggregated ownership stakes of pre-IPO sponsors in the firm are non-zero prior to the transaction, and zero after the transaction. Although it should be recognised that non-sponsors can do a clean-up trade as well, there is some uncertainty as to whether they will hold the stake for long-term investment or offload it in the aftermarket. With financial sponsors, market participants will be more certain that the stakes will come up for sale in the near term, making the effects we want to analyse more pronounced in those cases. Thus, this part of the analysis is limited to 721 sponsor-related transactions, meaning that all other transactions will be eliminated from the sample when estimating effects. Therefore, we cannot, and will not, say anything about clean-up trade effects for non-sponsored selldowns.

5.2 Calculation of abnormal returns and discounts

We adjust individual stock returns for returns in the underlying sector index in which company i belongs. We calculate abnormal returns as follows:

$$AR_{it} = R_{it} - R_{It} \tag{3}$$

Where AR_{it} is the abnormal return on company i on day t, R_{it} is the return on company i on day t and R_{It} is the return on sector or country index I, to which company i belongs, on day t. R_{it} is defined as the percentage price change over a given period, and is calculated by Dealogic for all transactions. Dealogic calculates returns from the offer price, meaning that discounts inflate returns. We adjust for this by using the closing price from the day prior to the selldown for returns calculations (net_ret in the models). R_{It} is defined as the percentage price change over the same period for the sector or country index in which firm i belongs. We use daily price returns for the MSCI sector and country indexes obtained from Bloomberg to adjust these returns. Dividends have not been adjusted for when calculating stock returns in the sample. The problem increases with the time horizon, as the likelihood of a dividend payment increases with time. On 1-day or 1-week basis, the problem is nearly non-existent. In order to mitigate this problem, the benchmark indexes are also calculated using raw returns net of dividends. In sum, as our maximum time horizon is one year, dividends should not bias our results in any significant way.

First-day abnormal returns are measured as the change in price from offer to close on the first day of trading after the transaction. The 1-day period limits movements in the underlying benchmark as daily expected returns are close to zero (Fama, 1998). Previous research has generally used either buy-and-hold abnormal returns (BHAR) or cumulative abnormal returns (CAR). Fama (ibid.) argues that both methods have theoretical and statistical shortcomings, with BHAR being more problematic for statistical inference when long-run returns are considered. Given the fact that we only have prices at fixed intervals after the transactions, we are unable to calculate the return as a compounded abnormal return (CAR), meaning that we can only calculate returns as buy-and-hold abnormal returns (BHAR). Given a time horizon of maximum one year, compounding monthly returns are likely to deviate only in a very insignificant way from the BHAR method.

With respect to the measurement of discounts, this paper considers only selldowns as overnight transactions, meaning that announcement and pricing happens while markets are closed. Therefore, the transaction happens in the complete absence of outside influences, and can be considered a “clean” data point. Some practitioners argue that the discount should be calculated both in relation to the closing price, but also in relation to a volume-weighted average of the price in the period leading up to the deal. The rationale is that if the share price is stable, but then increases just before the deal is announced, investors are likely to require a larger discount, and vice versa. However, we will base our discount calculation on the “last close” methodology; this is due to data availability of trading price and volume for the sample as well as established academic practice (see e.g. Bowen et al. (2008), Dempere (2012), S. Zhang (2005)). Further, in a sizeable sample, and given that average expected returns are near-zero over a couple of days, we would expect this to have a zero-mean and introduce no bias into our results.

5.3 Econometric model specifications and inference

We use a range of common econometric estimation techniques for the analysis. Mostly, we use the conventional ordinary least squares (OLS) regression model with multiple regressors, which provides the best linear unbiased estimator (the model is said to be “BLUE”) if a certain set of conditions are met (Stock and Watson, 2012).

In some cases, however, these criteria are not met. It is relevant to go through these assumptions, and how they may be violated, to understand the steps we take to circumvent potential biases. We will not go through the assumptions of variation in independent variables, and greater number of observations than parameters estimated, as these are fully satisfied.

(I) Normal distribution of the error term. If nonnormalities and outliers are severe, the OLS estimator will be biased. We identify nonnormalities that are likely to make the error term severely nonnormal by using the Shapiro-Wilk and Kolmogorov-Smirnov tests, and further analyse skewness and kurtosis of the sample distribution. We use the QR model where the issue is severe as it is semiparametric, and thus makes no assumptions about the population distribution. This makes it robust to the sort of nonnormalities that will bias the OLS estimator.

(II) The independent variables are non-stochastic variables that are uncorrelated with the error term. This assumption is violated if we have omitted variable bias, endogeneity or simultaneous causality. Although we cannot positively rule out this being the case, we use a range of control variables and different model specifications to limit the potential threat from violation of this assumption.

(III) Zero conditional mean, which states that the error term has an expected value of zero for all values of the independent regressors. As in (II) this issue can arise from endogeneity of independent regressors or omitted variables that causes variation in the error term. Again, this is dealt with through the inclusion of control variables and different model specifications.

(IV) The model is linear in its parameters, meaning that the dependent variable is a linear function of the independent variables and the error term. For some of the models, we do expect this assumption to be violated. Logarithmic transformation of variables or nonlinear estimation techniques is used where this is the case.

(V) Homoscedasticity of errors, meaning that errors have constant variance across observations. We use the White test to identify this potential problem, and use standard errors and model specifications that are robust to heteroscedasticity where we cannot assume homoscedasticity.

(VI) No autocorrelation of errors, meaning that the random variable u_i is to be statistically independent for all observations. This could be the case if returns data show significant momentum effect. We test for this using the Durbin-Watson test to determine if we need to take further steps to adjust for autocorrelation.

(VII) No multicollinearity, which is violated if two regressors are highly correlated. We test for this by analysing the Pearson correlation matrix, Eigenvalue and condition index.

5.3.1 (I) Non-normalities

Standard econometric models like the ordinary least squares (OLS) regression model assumes that the conditional distribution is a normal distribution. When we consider returns and discount as the dependent variable this assumption could be violated. Returns data tend to become positively skewed, more so for longer time horizons as returns compound (Fama et al., 1996). Intuitively, returns cannot go lower than -100% while the is no theoretical upper limit on returns, leading to a positive skew for longer-term returns. This is confirmed in earlier studies of the distribution of longer-term BHAR which has been found to be positively skewed, leading to negatively skewed sampling distribution of t (Barber and Lyon, 1997). The studies that find this positive skew to be problematic for inference typically has a longer time horizon than this paper. Short-term returns, specifically daily returns, have also been shown to deviate from the normality assumption (Brown and Warner, 1985). We test data using the Shapiro-Wilk and Kolmogorov-Smirnov tests to identify non-normalities. These tests lead us to reject the null hypothesis that the sample data comes from a normally distributed population for returns on all time horizons and for discounts. The apparent nonnormalities are further confirmed through computation of skewness and kurtosis for the dependent variable, which, for the most part, deviates significantly from 0 and 3, respectively, that is defined as a normal distribution (Stock and Watson, 2012) with short-term return data deviating the most. While this in itself does not make our estimators inefficient, we need to address this with respect to statistical inference from the results as the error term is likely to suffer from these nonnormalities. Our main concern is misspecified t-statistics that could lead us to falsely reject a true hypothesis, or fail to reject a false hypothesis. Previous research has addressed this issue through a range of different approaches.

Lyon et al. (1999), Khurshed et al. (2007) and others use an adjustment of the t-statistic to make inference more reliable. The specific method is the bootstrap skewness-adjusted t-statistic, an approach originally developed by Johnson (1978). The idea is to correct the t-statistic through an estimated coefficient of skewness in the distribution to reduce the probability of type 1 errors, and to make it more powerful in other cases. This approach would likely eliminate the skewness bias in a random sample, but is unlikely to do so for stock returns which tend to show a non-random clustering previously described. Thus, as Brav (2000) points out, this method does not eliminate misspecification in samples where returns overlap, which is also the case in this paper. In sum, the bootstrap skewness-adjustment of t-statistics is a complicated approach that is unlikely to effectively address the issue at hand. Therefore, this approach will not be used. The Wilcoxon rank-sum test has been used in previous research on SEOs and IPOs as a nonparametric alternative to the two sample t-test (see e.g. Chambers and Dimson (2009), Intintoli, Jategaonkar, et al. (2014), S. Zhang (2005)). The test makes no assumptions about the distributions, and usually tests the null hypothesis of equal median rather than mean (Hogg and Tanis, 2006). When we use this test to compare medians, say between discount for sponsor-backed and non-sponsored selldowns, we cannot include control variables. The sample data shows significant differences in firm characteristics and timing of selldowns between the two groups, meaning that statistical inference from the Wilcoxon test could be attributable to factors other than the one analysed. Thus, we deem this method inappropriate for the purpose of this paper.

Alongside the OLS models, we also use another linear estimator that has been proven to cope better with skewness and other non-normalities of the error process. Here, we estimate the quantile regression model (QR). QR deviates from the OLS model in that it minimises the absolute sum of residuals, $\sum_i |e_i|$, rather than the sum of squared residuals, $\sum_i e_i^2$, and allows for asymmetric penalties for over- and underprediction (Koenker and Hallock, 2001). It extends the OLS model, which fits data based on the conditional mean, to estimating coefficients by conditional quantiles of the response variable. The QR estimator for quantile ρ minimizes the following objective function:

$$Q(\beta_\rho) = \sum_{i:y_i \geq x_i'\beta}^{N} \rho \mid y_i - x_i'\beta_\rho \mid + \sum_{i:y_i < x_i'\beta}^{N} (1-\rho) \mid y_i - x_i'\beta_\rho \tag{4}$$

The QR estimator is semiparametric, in that it makes no assumptions about the parametric distribution of the error process, where the OLS model assumes normal distribution. This makes the model more robust to non-normal distributions and outliers than the OLS estimator, both of which are likely to be found when using returns data (Fama et al., 1996). The model is particularly useful when coefficients are dependent on the distribution of the response variable, which is likely to be the case for returns data. We use a bootstrap approach for computation of confidence interval using the resampling method as computed by the SAS software with the default option of 200 repeats. Specifically, SAS employs the Markov Chain Marginal Bootstrap as developed by He and Hu (2002). This method has proven robust to heteroscedasticity of the error term, in addition to robustness to outliers and non-normalities. Further, the method is stable in sample sizes like the ones used in this paper. We estimate the model for 19 distinct quantiles of the dependent variable, equally separated by 0.05 starting at the 0.05th quantile to the 0.95th quantile, and plot the corresponding confidence intervals for the quantile range. For each of the quantiles we also report the Wald test statistic as a robustness check of significance levels. As with the bootstrap errors, the Wald test has proven robust to heteroscedasticity of the error process (Koenker and Machado, 1999). Thus, we report both tests for all QR models. Using the QR model in addition to the conditional mean estimator in the OLS model ensures that the results are robust to covariates that are dependent on the distribution of the response variable.

5.3.2 (II)-(III) Control variables

We control for factors that could bias our regression estimates through the inclusion of a range of control variables. Some of the control variables are used in all models while others depend on the effects we are trying to estimate. All models include controls for **country effects**, **industry effects**, **time effects**, **selldown cycle effects** and **size effects**. Additional controls are used where applicable and the reasoning for the inclusion of a given variable will be explained for the models in which they are included. The reasoning behind the five controls included in all models, in addition to statistically significant covariates, will be given in this section, while a only a brief definition of additional control will be given here.

Country effects are included to control for country-specific effects. Although all markets in our sample are developed, and thus likely to be similar in many aspects, some differences could arise from e.g. corporate governance structure. We control for country effects by including a vector of dummies, one for each country in the sample.

Industry effects are integral in capturing industry-specific effects that could impact the results. These effects are controlled for through the inclusion of a vector of dummies for each of 27 identified industries the sample companies operate within. We choose narrowly defined industries, rather than a broader sector division, to control for effects as specifically as possible. Companies in related industries are likely to exhibit similar market risk, leverage ratios and industry maturity. Thus, it is argued that much of the company-specific risk is captured by these controls.

Having a time period that includes the inherent ups and downs of equity markets allows us not only to estimate effects at a given point in the cycle. IPOs and selldowns tend to cluster in hot markets, when investor optimism and valuations are high. It has previously been shown that these periods have significant effect on returns and underpricing Loughran and Ritter (2000). If transactions are clustered in periods where exogenous factors are causing the effects we are trying to estimate, the results will be biased. Thus, we control for this effect by including **time effect** controls, with the aim of capturing the effect of market sentiment at the time of the transaction. We use year dummies to capture the timing effect, which is a commonly used approach in the literature (see e.g. Cline, Fu, Tang, and Wiley (2012), M. Ferreira and Laux (2016), Li and Zhuang (2012), S. Zhang (2005)).

In addition to the controls for industry effects, we also control for **size effects** as this has been shown to have a strong relationship with returns Fama and French (1993). This allows us to control for another parameter that contains information about expected returns, and thus is likely to influence the results. The chosen control variable is the natural logarithm of the given company's market capitalisation just prior to the deal announcement. Logarithmic transformation is used to limit the impact of outliers, and as the size effect is likely to have a non-linear relationship with the variables of interest.

Selldown cycle effects are defined as the effect of how many selldowns have preceded the particular selldown. We include all selldowns as stand-alone in our dataset, rather than a series of selldowns from a particular company. This method could introduce bias as we do not differentiate between if a selldown is the first in a longer series, or the 9th. To control for this, we include dummy variables indicating what number selldown by company i a given selldown is.

Bookrunner effect is included for models with discount as the dependent variable to capture the effect on discount of having more bookrunners on a selldown. We calculate the variable as the natural logarithm of deal value in EUR divided by the total number of banks involved in the transaction:

$$\log syndicateint = \log\left(\frac{Deal\ value\ in\ EUR}{\#\ banks\ involved\ in\ the\ selldown}\right) \tag{5}$$

This serves as a proxy for the certification effect of having reputable bookrunners behind the transaction, which has previously been shown to decrease underpricing and discounts (Silva and Bilinski, 2015). Further, having more banks involved in the transactions also implies that a larger investor base is reached in the bookbuilding process, which is also likely to increase demand, in turn decreasing discount. We set the number of banks involved in relation to deal value as the two are likely to be an increasing function of each other. However, this relationship is likely non-linear as there is an upper bound on how many banks that are involved in each transaction, regardless of the size of the transaction (20 in our sample). Therefore, we transform the variable logarithmically.

changetoIPO is the percentage change in market capitalisation from the offering price at the IPO to the offering price at the selldown. Market capitalisation is used rather than share price to capture the effect of potential stock splits or dilution from e.g. options and warrants.

percsold is the number of shares sold as percent of the total number of shares outstanding. This is included to control for liquidity effects as a larger impact on underpricing and short-term excess returns should be expected, all else equal, when a larger share of the company is sold at once.

log*daysipo* the natural logarithm of the number of days since the given company went public at the time of the selldown. This variable is included to control for effects of having a longer trading record, which could reduce perceived risk. Also, patterns of abnormal performance following the IPO are likely to be a function of time, also to be captured by this variable.

We do not control for some important company characteristics due to data availability. Risk and company quality proxies such as leverage- and return ratios are not directly controlled for in our models. This could potentially bias results through misspecification of expected returns, which will compound over time. We argue that a combination of industry and size controls, and a time period that does not exceed 1 year, limits the threat to validity of our results. When considering models with dependent variables other than returns, the threat to validity is not a matter of misspecification of returns, but rather that we do not correctly identify some factors that causes variation in both the dependent- and independent variables. We cannot reject that this is in fact the case. However, a range of robustness checks and other control variables serve to improve the validity of our results.

5.3.3 (IV) Non-linearity

For some of the hypothesis testing, the data we are fitting have nonlinear relations. As mentioned, we use logarithmic transformation of variables where this is the case. A standard test for nonlinear relationship is regressing the squared independent variable on the dependent variable, which has been used in this paper. Further, logarithmic transformation has been used where outliers have been detected.

In other hypothesis tests, using linear estimation techniques like the OLS and QR models is not feasible. In models with a binary dependent variable, namely when we are estimating the probability of owners doing a selldown shortly after lock-up expiry, we use a probit model. Linear estimation procedures when the dependent variable is binary can yield predicted values above 1 and below 0, implying probabilities greater than 100% or sub-zero, respectively. In the probit model, the predicted value is a z-score, which, through the cumulative standard normal distribution, translates into a probability between 0 and 1:

$$P(Y = 1 \mid X_1, \cdots, X_K) = \phi(\beta_0 + \beta_1 X_1 + \cdots + \beta_K X_K) \tag{6}$$

Where $P(Y = 1 \mid X_k)$ is increasing in X_k for all $\beta_k > 0$, and $0 \leq P(Y = 1 \mid X1, \cdots, X_k) \leq 1$ for all X_k. The model is estimated using maximum likelihood, giving us the regression coefficients that maximises the probability of matching the real data (Stock and Watson, 2012).

5.3.4 (V) Heteroscedasticity

One condition for the OLS model to be BLUE is homoscedasticity, meaning that the variance of errors is constant across observations. There is strong evidence that volatility in stock returns varies over time (Schwert and Seguin, 1990), which in turn causes heteroscedasticity. This could also be the case for discounts given the previously found effects on underpricing in high-volume periods (Loughran and Ritter, 2002). In order to identify if this is the case in the sample data, we test for heteroscedasticity when estimating the models. This is done using the White test for each of the models White (1980), which tests the following null hypothesis:

$$H_0 : \sigma_i^2 = \sigma^2 \ for \ all \ i \tag{7}$$

That is, we reject the null of the standard errors being homoscedastic if the variance of errors is not constant across observations. While heteroscedasticity does not bias the OLS estimator, the reported standard errors will be biased, leading us to potentially rejecting a true hypothesis or fail to reject a false one. That is, we will still use the OLS estimator but the reported standard errors will be heteroscedasticconsistent where applicable, as computed by the SAS software. In addition, when estimating the QR model we report only bootstrap standard errors and the Wald test when errors are likely to be heteroscedasticity.

5.3.5 (VI) Autocorrelation

Some evidence of autocorrelation of both short-term and long-term returns has previously been found Fama (1991). Specifically, stock prices have been shown to take "swings" away from fundamentals over time, although short-term returns have often been shown to have economically insignificant autocorrelation. We use the Durbin-Watson test to determine if correction for autocorrelation is needed for any of the dependent variables in our models. Test results are not reported, but have shown no signs of autocorrelation for any of the estimated models. Thus, we deem our data free of this potential issue.

5.3.6 (VII) Multicollinearity

Using a larger number of control variables in the models could introduce multicollinearity if these variables are highly correlated. This generally do not make estimators biased or inefficient, but can result in larger standard errors, and thus less powerful t-statistics, for the multicollineary variables. We test for multicollinearity of variables using the Pearson correlation coefficients and the Eigenvalue and condition index as computed by SAS. We find no collinearity of problematic proportions, with the largest correlation coefficient between non-vector variables being 0.35.

5.4 Measures of fit

Measures of fit like R^2 and the Akaike Information Criterion will not be reported for the estimated models. These statistics are generally used to determine whether a given model is a good predictor of a certain dependent variable. As the purpose of this paper is not prediction as such, but rather marginal effects from a set of events or attributes these statistics will not provide any meaningful information.

5.5 Datasets

We use different datasets depending on the hypothesis in question. These datasets either include or exclude selldowns depending on 3 parameters: (1) sponsor-backed or non-sponsored, (2) lock-up provision or no lock-up provision, and (3) data available for discount or no data available for discount. In total, we use 7 different datasets. It will be specified for each of the hypothesis which dataset we use for modelling and testing. The table below provides an overview of the datasets:

Table 2: Overview of data sets. "x" indicates inclusion

	Obs.	**SB**	**NS**	**No discount**	**Lock-up**	**No lock-up**
Luspons	580	x		x	x	
Luspons_disc	562	x			x	
Lunonspons	277		x	x	x	
Lunonspons_disc	262		x		x	
full	1,232	x	x	x	x	x
Luclean-up_full	857	x	x	x	x	
spons_disc	721	x			x	x
nonspons_disc	459		x		x	x
full_disc	1,180	x	x		x	x

6 Sample statistics

Table 3 shows the full sample of 619 firms conducting their initial public offering between January 1, 2000 and February 16, 2015 and subsequently conducting one or more selldowns of secondary shares. The total number of selldowns in the sample is 1,232 between February 16, 2000 and February 16, 2016. Out of the 1,232 selldowns, 745 were sponsor-backed and 487 non-sponsored. The combined proceeds of the selldowns were more than 310 billion euros, of which 186 billion euros went to financial sponsors and 124 billion euros to owners of non-sponsored companies. The average market capitalisation of sponsored and non-sponsored companies at the time of the selldown was approximately 2.6 billion euros and 4.4 billion euros, respectively.

Table 3: Overview of selldowns by year

	Number			Aggregate deal value (EURm)			Average market cap (EURm)			IPO
Year	All	SB	NS	All	SB	NS	All	SB	NS	# IPOs
2000	6	2	4	331	106	225	1,053	514	1,322	57
2001	24	3	21	4,432	121	4,311	3,828	421	4,314	37
2002	17	2	15	5,423	92	5,331	4,367	679	4,858	26
2003	33	15	18	8,003	2,413	5,590	4,563	1,572	7,055	16
2004	64	32	32	15,684	5,577	10,107	3,100	1,599	4,600	52
2005	69	32	37	14,090	4,404	9,686	2,973	1,302	4,419	59
2006	90	52	38	17,247	9,742	7,505	2,222	1,754	2,862	71
2007	105	58	47	18,908	10,411	8,497	3,171	1,480	5,257	51
2008	31	18	13	3,676	2,010	1,666	1,637	1,128	2,341	2
2009	47	20	27	6,726	2,266	4,460	1,371	1,092	1,578	13
2010	68	44	24	10,751	8,085	2,666	1,913	1,857	2,015	39
2011	67	47	20	16,498	14,168	2,330	2,899	2,806	3,118	34
2012	88	58	30	35,649	17,753	17,896	4,715	2,993	8,043	34
2013	139	93	46	44,383	30,874	13,509	4,577	3,337	7,082	56
2014	172	117	55	44,553	32,845	11,708	3,030	3,023	3,043	71
2015	207	149	58	62,862	44,874	17,988	3,682	3,548	4,024	1
2016	5	3	2	1,552	716	836	4,925	2,610	8,399	0
Total	**1,232**	**745**	**487**	**310,768**	**186,457**	**124,311**	**3,294**	**2,587**	**4,375**	**619**

The volume of selldowns fluctuates each year, with extraordinarily low activity in 2001-2003 following the IT bubble as well as after the financial crisis in 2008. Approximately three quarters of the total deal value in the sample occurs after 2009, which is a consequence the sample structure where more companies are added as time passes. It may also, to some extent, reflect the increasing popularity of accelerated selldowns as an exit strategy. Observing the developments in deal value, it seems that there is a tendency for sponsor-backed activity to pick up more slowly after crises, and then bounce back in years of high activity, which is consistent with the "hot market" theory.

The next table summarises the number of selldowns and aggregate proceeds in each of 27 industries. We note that, both on a volume and a value basis, selldowns are not equally distributed over the industries. The top 5 industries by value make up 45% of the total proceeds in the sample, while bottom 5 make up less than 2%. In most cases, the distribution of selldowns in a given industry is not symmetric for sponsor-backed and non-sponsored companies. Proceeds from selldowns in companies operating in industries such as healthcare, automotive, chemicals as well as most consumer-facing industries are characterised by sponsor ownership, while companies within utilities, insurance, aerospace and mining generally do not have sponsor affiliation.

Table 4: Overview of selldowns by year

	Number			Aggregate deal value (EURm)			
Main sector	**All**	**SB**	**NS**	**All**	**SB**	**NS**	**% SB**
Oil & Gas	77	44	33	19,018	9,182	9,836	48%
Agribusiness	5	0	5	795	0	795	0%
Chemicals	44	40	4	13,247	12,766	481	96%
Forestry & Paper	6	4	2	702	537	165	76%
Metal & Steel	29	25	4	5,048	4,552	496	90%
Mining	13	1	12	2,422	149	2,273	6%
Aerospace	15	7	8	7,823	842	6,981	11%
Auto/Truck	43	41	2	8,366	7,783	583	93%
Constr./Building	26	15	11	4,650	3,152	1,498	68%
Defense	7	6	1	1,052	732	320	70%
Machinery	22	18	4	3,977	3,648	329	92%
Professional Services	71	54	17	16,163	14,961	1,202	93%
Transportation	56	27	29	16,215	5,232	10,983	32%
Consumer Products	28	20	8	5,010	2,869	2,141	57%
Dining & Lodging	28	27	1	10,442	10,429	13	100%
Leisure & Recr.	29	23	6	7,234	6,947	287	96%
Publishing	13	5	8	3,300	1,503	1,797	46%
Retail	98	66	32	27,603	21,329	6,274	77%
Textile	2	0	2	272	0	272	0%
Food & Beverage	15	10	5	3,215	2,564	651	80%
Healthcare	68	48	20	16,192	14,466	1,726	89%
Finance	109	43	66	22,618	8,599	14,019	38%
Insurance	51	21	30	25,249	2,209	23,040	9%
Computers & Electr.	218	137	81	46,439	33,418	13,021	72%
Telecommunications	70	32	38	19,687	10,966	8,721	56%
Utility & Energy	51	8	43	15,793	2,007	13,786	13%
Real Estate/Property	38	23	15	8,236	5,615	2,621	68%
Total	**1,232**	**745**	**487**	**310,768**	**186,457**	**124,311**	**60%**

The geographic distribution of proceeds is also unevenly distributed. 42% of total proceeds went to owners of U.S. companies, with France, Germany and United Kingdom also representing a large share of the sample. The main part of proceeds from selldowns in Danish, Swedish, Belgian and U.S. companies belong to sponsors, with sponsor-backed transactions clearly driving the aggregate selldown value more so than in other geographies. With the exception of Japan, Asia-Pacific countries have lower proportion of sponsor-backed companies. It is important to note how heavily represented the U.S. is within sponsor-backed companies. Approximately 56% of the total value of sponsor-backed selldowns were in U.S.-based companies, but the U.S. is home to only 14% of non-sponsored selldown value in the sample.

Table 5: Overview of selldowns by country

	Number			**Aggregate deal value (EURm)**			
	All	**SB**	**NS**	**All**	**SB**	**NS**	**% SB**
Austria	9	3	6	3,146	177	2,969	6%
Belgium	7	6	1	2,233	1,696	537	76%
Denmark	16	10	6	6,053	5,379	674	89%
Finland	1	0	1	50	0	50	0%
France	70	34	36	28,670	11,201	17,469	39%
Germany	89	52	37	33,291	16,505	16,786	50%
Ireland	8	2	6	1,130	256	874	23%
Israel	2	0	2	140	0	140	0%
Italy	41	17	24	6,619	3,004	3,615	45%
Netherlands	20	10	10	10,450	4,894	5,556	47%
Norway	34	9	25	7,517	664	6,853	9%
Portugal	9	0	9	3,228	0	3,228	0%
Spain	39	11	28	8,491	3,244	5,247	38%
Sweden	47	34	13	4,308	2,756	1,552	64%
Switzerland	19	7	12	2,359	1,164	1,195	49%
United Kingdom	208	114	94	31,083	18,007	13,076	58%
Canada	43	25	18	10,177	4,982	5,195	49%
United States	444	365	79	122,276	104,846	17,430	86%
Australia	55	26	29	7,122	3,534	3,588	50%
Hong Kong	38	5	33	16,069	1,039	15,030	6%
Japan	15	9	6	3,833	2,780	1,053	73%
New Zealand	6	3	3	874	214	660	24%
Singapore	12	3	9	1,649	115	1,534	7%
Total	**1,232**	**745**	**487**	**310,768**	**186,457**	**124,311**	**60%**

The table below summarizes statistics on the main metrics of this paper. Average lock-up time including selldowns with no lock-up provision is similar for sponsor-backed and non-sponsored transactions. Average lock-up duration is approximately 40 days more for non-sponsored selldowns when eliminating selldowns without lock-up provision. On average, in cases where the selldown had a lock-up provision, sponsors conducted selldowns 391 days sooner after the lock-up provision expired. The difference in lock-up provision characteristics becomes even more evident when comparing medians. The median number of days between lock-up expiry and selldown is 2.6x higher for non-sponsored transactions.

Table 6: Overview of sample statistics. First-day and 1-year returns is excluding discount

	All	**SB**	NS
# Unique companies	619	371	248
Average lockup days for all seldowns	93.1	93.3	93.0
Median lockup days for all seldowns	90.0	90.0	90.0
Average lockup days for selldowns with lockups	133.9	119.8	163.5
Median lockup days for seldowns with lockups	180.0	120.0	180.0
Average days selldown from lockup (lockup deals only)	470.8	344.5	735.3
Median days selldown from lockup (lockup deals only)	175.0	124.5	328.0
Average days between IPO and seldown	1,053.8	871.8	1,333.3
Median days between IPO and seldown	706.5	370.5	414.5
% selling within 10 days of lockup	18.7%	20.7%	14.4%
% selling within 5 days of lockup	13.4%	15.3%	9.4%
Average discount %	-3.7%	-3.4%	-4.1%
Median discount %	-3.3%	-3.2%	-3.5%
Average net first-day abnormal returns %	-2.5%	-2.5%	-2.6%
Median net first-day abnormal returns %	-2.4%	-2.5%	-2.4%
Average net 1-year abnormal returns %	2.6%	5.0%	-1.2%
Median net 1-year abnormal returns %	0.9%	4.0%	-2.6%
Average number of selldowns	2.0	2.0	2.0
Median number of selldowns	1.0	2.0	1.0

Similarly, 20.7% of sponsor-backed transactions occurred within 10 days of expiry of the lock-up provision compared to 14.4% for non-sponsored selldowns. We also see smaller average and median discount for financial sponsors, with a difference of 0.7 percentage point and 0.3 percentage points, respectively. The pattern is less pronounced for net first-day abnormal returns, with an average 0.1 percentage point greater first-day returns for non-sponsored companies. When using median, the relationship is the opposite, with 0.1 percentage point higher net first-day abnormal returns for sponsor-backed companies. The difference between sponsor-backed and non-sponsored selldown is remarkable when comparing net sector-adjusted 1-year returns. Net returns (price change from last close before the selldown to first close after) are positive for sponsor-backed companies and negative for non-sponsored companies, with a difference of more than 6 percentage points for both average and median. These numbers reflect returns to an investor that does not partake in the selldown. An investor buying at the offer price would get positive 1-year abnormal returns for both selldown types (adding back the discount). The average number of selldowns is nearly identical, while median shows financial sponsors having more selldowns for each company in the sample.

7 Results and findings

In this section, we specify and estimate the econometric models we have applied for hypothesis testing for the three groups. We go through the results of each model and weigh the evidence supporting or opposing the given hypothesis. A thorough discussion of the results and their implications can be found in the subsequent discussion section.

7.1 Hypothesis group 1: The effect of sponsor-backing on returns and discounts

7.1.1 Hypothesis 1a

H1a: Sponsor-backed selldowns have smaller discounts than non-sponsor-backed selldowns

Econometric models applied:

1. Ordinary least squares
2. Ordinary least squares including control for clean-up trade
3. Quantile regression
4. Quantile regression including control for clean-up trade

We run several models in testing this hypothesis. First, we use a standard OLS model without controlling for clean-up trades. Next, we run the same model controlling for clean-up trades. Further, we estimate QR models with the same specifications. For all four models, the dependent variable is discount from the last trade before the offer to the offer price. The independent variable of interest is a binary variable indicating 1 if the selldown is sponsor-backed and 0 otherwise. Additional regressors are included only as control variables, and their estimated coefficients will not be interpreted in the models. We use the dataset with all selldowns where discount data is available for this model. Specification of the model is as follows:

$$
\begin{aligned}
Discount_i =& \beta_0 + \beta_1 selldown\_spons_i + \beta_2 percsold_i + \beta_3 changetoipo_i \\
& + \beta_4 \log daysipo_i + \beta_5 \log syndicateint_i + \beta_6 \log selldown\_mcap_i \\
& + \beta_7 country_i + \beta_8 industry_i + \beta_9 year_i + \beta_{10} selldno_i + \epsilon_i
\end{aligned} \tag{8}
$$

And the model controlling for the effect of clean-up trades:

$$
\begin{aligned}
Discount_i =& \beta_0 + \beta_1 selldown\_spons_i + \beta_2 percsold_i + \beta_3 changetoipo_i \\
& + \beta_4 \log daysipo_i + \beta_5 \log syndicateint_i + \beta_6 \log selldown\_mcap_i \\
& + \beta_7 clean\text{-}up_i + \beta_8 country_i + \beta_9 industry_i + \beta_{10} year_i + \beta_{11} selldno_i + \epsilon_i
\end{aligned} \tag{9}
$$

Where ϵ_i is the error term. For control variables we have included *percsold* as a proxy for the supply shock to the price coming from the selldown. Building on supply-demand theory, we control for the likely effect of investors demanding a lower price, and thus a larger discount, if a larger share of the company is for sale. *Changetoipo* is included to control for effects on discount of the stock price behaviour prior to the transaction, as previous research has produced evidence of a relationship between the two (Intintoli, Jategaonkar, et al., 2014). log*syndicateint* is included to capture the potential effect on discount of having more banks involved in the bookbuilding process, which is also likely to be correlated with sponsor-backing (sponsors have more banks involved in the selldown on average in the sample). Running the White test on the two OLS models, we cannot reject the null hypothesis that errors are homoscedastic and thus use the conventional standard errors for inference.

The coefficient estimate to *selldown_spons* is 0.6223, indicating a 0.6 percentage points decrease in discount when a selldown is sponsor-backed. The estimate is statistically significant at the 1% alpha level. Following the reasoning of hypothesis 2a, we estimate a model which includes a control for clean-up trades. We obtain similar results but with weaker magnitude and significance. The coefficient to *selldown_spons* is estimated at 0.4753 in this model with a p-value of 0.0464. Thus, the coefficient estimate is no longer significant at the 1% alpha level. More of the variation is now captured by the *clean-up* variable with a coefficient estimate that is positive and significant at the 10% alpha level. The positive correlation between *clean-up* and discount in this model provides evidence that sponsors reap some of the smaller discounts specifically from clean-up trades, which will be explored in greater depth in hypothesis group 2.

We use the same variables for the QR models and specify it as follows for company i at the ρth quantile without the control for clean-up trade:

$$\begin{aligned} Discount_i = &\beta_0^\rho + \beta_1^\rho selldown\_spons_i + \beta_2^\rho percsold_i + \beta_3^\rho changetoipo_i \\ &+ \beta_4^\rho \log daysipo_i + \beta_5^\rho \log syndicateint_i + \beta_6^\rho \log selldown\_mcap_i \\ &+ \beta_7^\rho country_i + \beta_8^\rho industry_i + \beta_9^\rho year_i + \beta_{10}^\rho selldno_i + \epsilon_i \end{aligned} \tag{10}$$

And for the model with clean-up trade control:

$$\begin{aligned} Discount_i = &\beta_0^\rho + \beta_1^\rho selldown\_spons_i + \beta_2^\rho percsold_i + \beta_3^\rho changetoipo_i \\ &+ \beta_4^\rho \log daysipo_i + \beta_5^\rho \log syndicateint_i + \beta_6^\rho \log selldown\_mcap_i \\ &+ \beta_7 clean\text{-}up_i + \beta_8^\rho country_i + \beta_9^\rho industry_i + \beta_{10}^\rho year_i + \beta_{11}^\rho selldno_i + \epsilon_i \end{aligned} \tag{11}$$

Running the QR model without *clean-up* yields results similar to the equivalent OLS model, though with less economically meaningful coefficient estimate at 0.3304 percentage points smaller discount for sponsor-backed selldowns. Further, this estimate is significant only at the 10% alpha level. Fitting the model for 19 distinct quantiles, we get a range of coefficient estimates of -0.2056 to 1.4532 with the estimate being negative only at the 0.95th quantile. The median estimate is significant at the 10% alpha level implied by the bootstrap standard errors and the Wald test. Taken together, evidence is strong that sponsor-backing has a positive effect on discount in the QR model. However, if we include *clean-up* in the QR model, we no longer have a statistical significant estimate for sponsor-backing, although it remains positive. The median regression estimate is 0.1171 with a range of -0.3665 to 1.3218 for the

Table 7: Hypothesis 1a OLS and QR regressions. Dependent variable is selldown discount

Variable	**OLS**	**OLS**	**QR (at 0.5)**	**QR (at 0.5)**
Std. Errors	Conv.	Conv.	Bootstrap	Bootstrap
Intercept	-5.20134	-5.18219	-5.40230	-5.52260
	(3.6099)	(3.60566)	(4.0419)	(3.4832)
Selldown_Spons	0.62230	0.47531	0.33040	0.11710
	(0.2256)***	(0.23834)**	(0.1867)*	(0.1979)
logselldown_mcap	0.93499	0.95583	1.02520	1.06000
	(0.15422)***	(0.15443)***	(0.1196)***	(0.1212)***
Perc_sold	-0.05823	-0.06246	-0.03800	-0.03780
	(0.01507)***	(0.01522)***	(0.0119)***	(0.0124)***
ChangetoIPO	-0.00094	-0.00098	-0.00080	-0.00050
	(0.00066)	(0.00066)	(0.0006)	(0.0006)
logdaysipo	-0.57380	-0.57643	0.66510	-0.67010
	(0.12023)***	(0.12010)***	(0.1066)***	(0.1040)***
logsyndicateint	-0.43102	-0.43761	-0.53050	-0.50890
	(0.14677)***	(0.14664)***	(0.1217)***	(0.1309)***
Clean-up		0.50396		0.59720
		(0.26597)*		(0.2138)***
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
No. observations	1,180	1,180	1,180	1,180

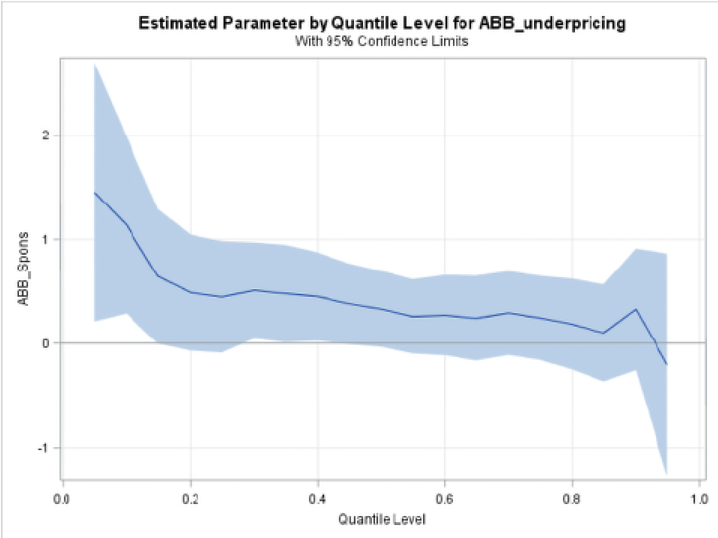

Figure 2: H1a QR regression without clean-up control

same quantile range as before. The median estimate is insignificant for both test statistics, and the coefficient is estimated close to zero for a large part of the quantiles. Looking at the plots in figure 3 confirms that sponsor-backing is now closer to zero, while clean-up is a significant predictor of discounts. This adds to the evidence found in the OLS model that the statistical relationship between sponsor-backing and discounts weakens when controlling for the effect of clean-up trades. In the QR model, the effect of sponsor-backing disappears altogether. This gives a high degree of certainty that we can allocate most of the effect on discount from sponsor-backing specifically to effects relating to clean-up trades.

In sum, the models seem to confirm the hypothesis that sponsor-backed selldowns have smaller discounts than non-sponsored selldowns. However, evidence indicates that much, or close to all, of this effect is attributable to clean-up trades that has a strong positive effect on discounts. This effect will be specified in appropriate models to isolate the effect of clean-up trades in later sections.

7.1.2 Hypothesis 1b

H1b: Sponsor-backed selldowns have greater 1-year abnormal returns as compared to non-sponsored selldowns

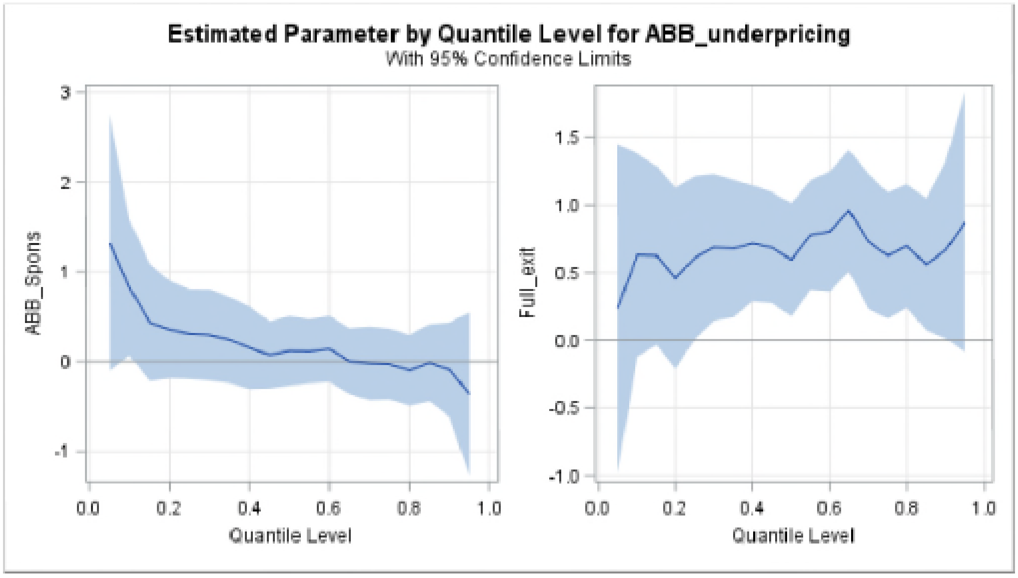

Figure 3: H1a QR regression with clean-up control

Econometric models applied:

1. Ordinary least squares with sector-adjusted returns
2. Ordinary least squares with sector-adjusted returns and control for clean-up
3. Quantile regression with sector-adjusted returns
4. Quantile regression with sector-adjusted returns and control for clean-up
5. Ordinary least squares with country-adjusted returns
6. Ordinary least squares with country-adjusted returns and control for clean-up
7. Quantile regression with country-adjusted returns
8. Quantile regression with country-adjusted returns and control for clean-up

For this hypothesis, we estimate various models. We estimate a total of four OLS models – with and without the *clean-up* variable using both sector- and country index-adjusted returns. In addition, we estimate four QR models with the same specifications as in the OLS models. All models have 1-year adjusted returns as the dependent variable, and a binary sponsor-backing variable as the independent variable of interest. We use the dataset with all selldowns where discount data is available, and specify the OLS model as follows for company i:

$$
\begin{aligned}
Return_{1y,i} =& \beta_0 + \beta_1 selldown\_spons_i + \beta_2 \log selldown\_mcap_i + \beta_3 country_i \\
&+ \beta_4 industry_i + \beta_5 year_i + \beta_6 selldno_i + \epsilon_i
\end{aligned} \tag{12}
$$

Similarly, we specify the OLS with clean-up control:

$$
\begin{aligned}
Return_{1y,i} =& \beta_0 + \beta_1 selldown\_spons_i + \beta_2 \log selldown\_mcap_i + \beta_3 clean\text{-}up_i \\
&+ \beta_4 country_i + \beta_5 industry_i + \beta_6 year_i + \beta_7 selldno_i + \epsilon_i
\end{aligned} \tag{13}
$$

Where $Return_{1y,i}$ is either the sector- or country index-adjusted returns. In order to estimate the predicted effect on returns from a selldown being sponsor-backed, we need to control for additional factors that could be correlated with returns and with being sponsor-backed, thus biasing our estimates if omitted. We include controls for factors that have been suggested in previous research to have a non-zero impact on expected returns. These are to a large extent captured by the 4 control variables for size, country, industry and year. Although some effect from time and returns since IPO could be expected, these controls have not been included. This is due to these two control variables having shown no explanatory power in preliminary tests, or impact on the results in each model in any significant way.

Testing for heteroscedasticity, we can reject the null hypothesis of homoscedasticity with a high degree of statistical significance, rendering it necessary to report heteroscedasticity-consistent standard errors.

The OLS model yields a parameter estimate of 6.8627 for *selldown_spons*, implying a positive impact of 6.9 percentage points on 1-year sector-adjusted abnormal returns from the time of selldown if the company is sponsor-backed. Using heteroscedasticity-consistent standard errors, we get a highly significant relationship with a p-value of 0.016. If we include the *clean-up* variable, in accordance with hypothesis 2c, we get increasingly positive and statistically significant results. The beta estimate for *selldown_spons* increases to 8.6673 translating into a positive impact of 8.7% on 1-year sector-adjusted abnormal returns. This result is statistical significant at the 1% alpha level. That is, we have separated the negative correlation

Table 8: Hypothesis 1b OLS regressions. Dependent variable is 1-year abnormal returns

Variable	OLS	OLS	OLS	OLS
Error	White	White	White	White
Intercept	-13.7788 (28.18243)	-13.1275 (28.12043)	-7.1303 (27.72525)	-6.49158 (27.70798)
Selldown_spons	6.86265 (2.84339)**	8.66726 (3.00320)***	6.18439 (2.88683)**	7.95413 (3.06162)***
logselldown_mcap	-0.06321 (1.21017)	-0.32577 (1.21342)	0.14271 (1.23222)	-0.11478 (1.23729)
Clean-up		-5.91777 (3.12706)*		-5.8034 (3.16306)*
Net ret adj	Sector	Sector	Country	Country
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
Observations	1,180	1,180	1,180	1,180

between *clean-up* and returns from the positive correlation with sponsor-backing to get a "cleaner" estimate of the effect of being sponsor-backed alone. Running the same OLS models with country index-adjusted abnormal returns yield results of roughly equal magnitude and significance, making the results robust to an alternative benchmark specification.

We specify the QR model without clean-up control as follows for company i at the ρth quantile:

$$\begin{aligned} Return_{1y,i} =& \beta_0^{\rho} + \beta_1^{\rho} selldown\_spons_i + \beta_2^{\rho} \log selldown\_mcap_i + \beta_3^{\rho} country_i \\ &+ \beta_4^{\rho} industry_i + \beta_5^{\rho} year_i + \beta_6^{\rho} selldno_i + \epsilon_i \end{aligned} \tag{14}$$

And for the model including the control variable for clean-up transactions:

$$\begin{aligned} Return_{1y,i} =& \beta_0^{\rho} + \beta_1^{\rho} selldown\_spons_i + \beta_2^{\rho} \log selldown\_mcap_i + \beta_3^{\rho} clean\text{-}up_i \\ &+ \beta_4^{\rho} country_i + \beta_5^{\rho} industry_i + \beta_6^{\rho} year_i + \beta_7^{\rho} selldno_i + \epsilon_i \end{aligned} \tag{15}$$

The QR estimator is statistically and economically significant at the median. The coefficient to *selldown_spons*, not including *clean-up*, implies a 6.6 percentage points impact on returns, and is highly significant for both bootstrap standard errors and the Wald test. The coefficient is positive across all estimated quantiles with the estimate being statistically insignificant at the 5% alpha level only at the 0.65th, 0.90th and 0.95th quantiles using bootstrap standard errors. The Wald test indicates highly significant coefficient estimates for all quantiles except the 0.90th and 0.95th. Thus, the QR model indicates that sponsor-backing has a significant effect on 1-year abnormal returns for all but the top quantiles of the distribution of returns.

If we adjust the model to include the *clean-up* variable, the effect is much like with the OLS models. The median coefficient estimate for *selldown_spons* is now 9.0974, indicating a larger positive effect of sponsor-backing when controlling for selldowns in which the sponsor exits. The estimate is highly significant at all estimated quantiles expect the 0.90th and 0.95th. The Wald test yields roughly the same significance lev-

Table 9: Hypothesis 1b QR regressions. Dependent variable is 1-year abnormal returns

Variable	QR (at 0.5)	QR (at 0.5)	QR (at 0.5)	QR (at 0.5)
Error	Bootstr.	Bootstr.	Bootstr.	Bootstr.
Intercept	4.3793 (58.3440)	-0.9106 (64.1511)	-10.8689 (46.6806)	-14.7432 (63.4555)
Selldown_spons	6.5821 (2.5830)**	9.0974 (2.8046)***	6.4543 (2.8034)**	8.7978 (2.7508)***
Logselldown_spons	0.4643 (0.9956)	0.3541 (0.9610)	1.7865 (0.9494)*	1.9035 (0.9980)*
Clean-up		-8.5216 (3.0648)***		-5.5412 (3.0895)*
Net ret adj	Sector	Sector	Country	Country
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
Observations	1,180	1,180	1,180	1,180

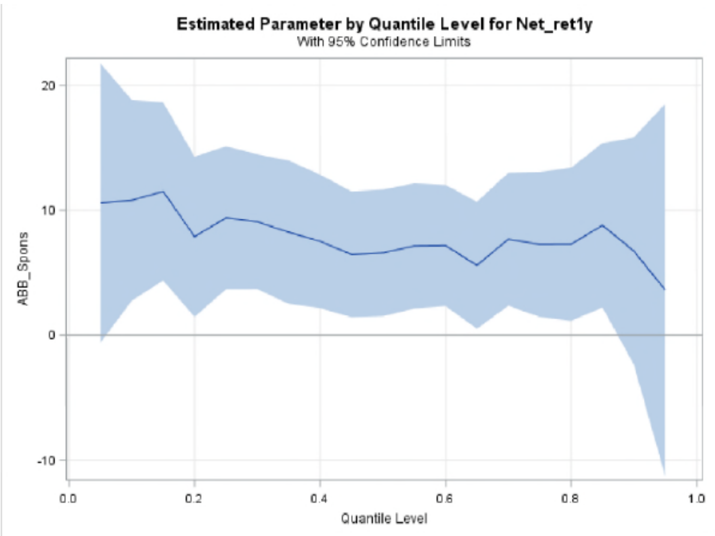

Figure 4: H1b QR regression with sector-adjusted returns without clean-up control

els as in the model without *clean-up*, with the same quantiles being significant. The *clean-up* variable has a median coefficient estimate of -8.5216, which is significant for both test statistics. However, for the full range of quantiles, the statistical and economic significance varies with point estimates being both positive and negative.

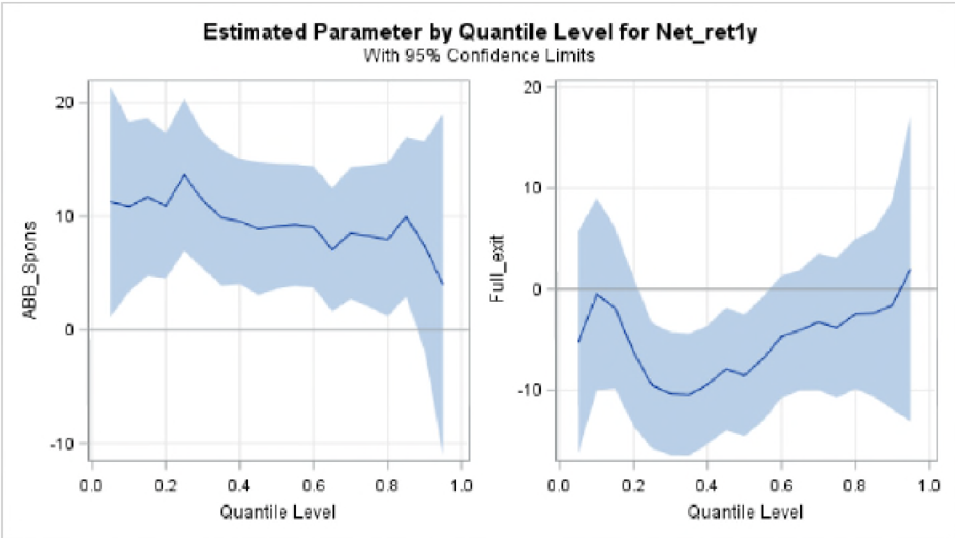

Figure 5: H1b QR regression with sector-adjusted returns and clean-up control

The significance of the *clean-up* variable is more ambiguous in this case with zero being included in the 95% confidence interval for a large part of the quantiles. In sum, there is weaker evidence here than in hypothesis 1a that *clean-up* has a statistically and economically significant relationship with the dependent variable. As for the OLS models, the QR estimators for country-adjusted abnormal returns are of equal statistical and economic significance. The median coefficient estimate with this dependent variable is 6.4543, and again highly significant for all three test statistics.

The models used for testing this hypothesis in aggregate provides strong evidence that sponsor-backed selldowns do have better aftermarket performance over a 1-year time horizon. The results are robust to the two returns adjustment methods, and is significant for most quantiles of the distribution of the dependent variable. We do find some evidence that this effect is weaker when clean-up trades are considered, but the statistical significance of this effect is markedly weaker. A more appropriate model specification will be used in later sections to investigate this effect further.

7.2 Hypothesis group 2: Clean-up trades

7.2.1 Hypothesis 2a

H2a: Discounts are smaller for clean-up trades than other sponsor-backed selldowns

Econometric models applied:

1. Ordinary least squares
2. Quantile regression

We test this hypothesis using an OLS regression with discount as the dependent variable, full sponsor exit as the independent variable, and a range of control variables. The dataset used includes all sponsor-backed selldowns with discount data. Due to data limitations on ownership stakes and full exit by non-sponsors, we exclude non-sponsor transactions from this part of the paper. We model the OLS estimator as follows:

$$\begin{aligned} Discount_i = &\beta_0 + \beta_1 clean\text{-}up_i + \beta_2 percsold_i + \beta_3 changetoipo_i \\ &+ \beta_4 \log syndicateint_i + \beta_5 \log selldown\_mcap_i + \beta_6 country_i \\ &+ \beta_7 industry_i + \beta_8 year_i + \beta_9 selldno_i + \epsilon_i \end{aligned} \tag{16}$$

The independent variable is a binary variable indicating 1 if the selldown marks a full exit of pre-IPO sponsors and 0 otherwise. Thus, the beta estimate can be interpreted as the level effect on discount from a selldown being sponsor-backed. We have included the same controls as with other models having discount as the dependent variable. *Percsold* again serves as a proxy for the supply shock to the price coming from the selldown, *changetoipo* is included to control for pre-selldown stock price behaviour, and log*syndicateint* serves as a demand-side proxy. Testing for heteroscedasticity, we get a p-value of 0.2770 which indicates that we cannot reject that the error term is homoscedastic.

Table 10: Hypothesis 2a OLS and QR regressions. Dependent variable is selldown discount

Variable	**OLS**	**QR (at 0.5)**
Error	Conv.	Bootstr.
Intercept	-9.17562	-10.0195
	(4.12562)**	(3.3663)***
Full_exit	0.49459	0.8540
	(0.27406)*	(0.2420)***
logABB_mcap	0.96226	1.0532
	(0.21738)***	(0.1606)***
Perc_sold	-0.05992	-0.0436
	(0.02047)***	(0.0158)***
ChangetoIPO	-0.00063834	-0.0009
	(0.00096641)	(0.0010)
logsyndicateint	-0.29053	-0.4101
	(0.19216)	(0.1385)***
logdaysipo	-0.38578	-0.2030
	(0.17081)***	(0.1488)
Country control	Yes	Yes
Industry control	Yes	Yes
Year control	Yes	Yes
Selldown # control	Yes	Yes
Observations	721	721

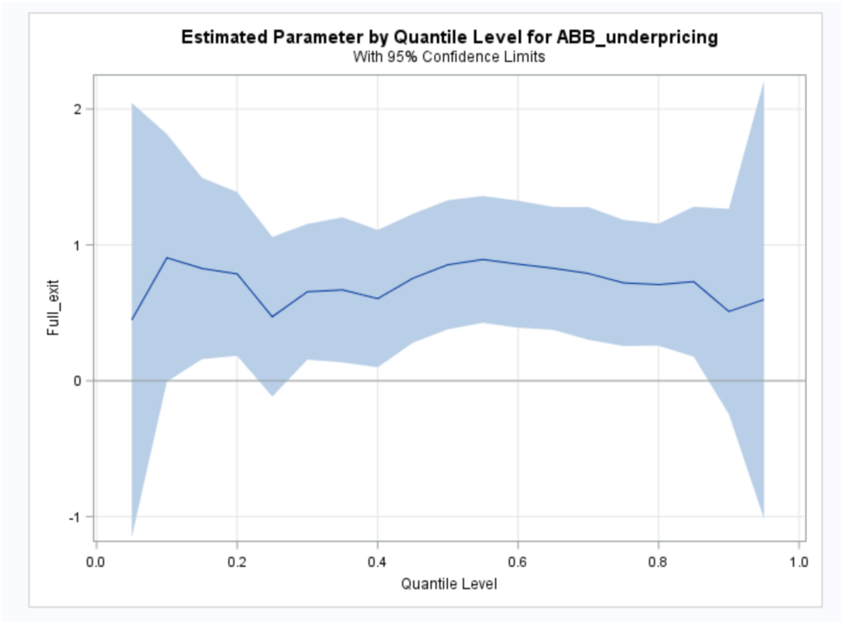

Figure 6: H2a QR regression

Running the OLS model, the coefficient estimate for *clean-up* is 0.4946 and is significant at the 10% alpha level with a p-value of 0.0716. This implies an economically significant change in discount for a sponsor-backed selldown when it is in the form of a clean-up trade. The discount is reduced, on average and holding other factors constant, by 0.5 percentage points for a dependent variable with a -3.5 percentage points mean value.

The QR model is specified as follows for company i at the ρth quantile:

$$\begin{aligned} Discount_i = &\beta_0^\rho + \beta_1^\rho clean\text{-}up_i + \beta_2^\rho percsold_i + \beta_3^\rho changetoipo_i \\ &+ \beta_4^\rho \log syndicateint_i + \beta_5^\rho \log selldown\_mcap_i + \beta_6^\rho country_i \\ &+ \beta_7^\rho year_i + \beta_8^\rho industry_i + \beta_9^\rho selldno_i + \epsilon_i \end{aligned} \tag{17}$$

The QR estimator gives a similar median coefficient estimate at 0.854 and a p-value of 0.0004. Using the bootstrap errors, the coefficient is statistically significant at the 5% alpha level for all estimated quantiles but for the outer ends of the distribution of the dependent variable, which is also the case for the Wald test. These results support the estimated effects in the OLS models. Taken together, we find strong evidence of a positive impact on discount from clean-up trades which confirms the hypothesis set out here.

7.2.2 Hypothesis 2b

H2b: First-day returns are less negative after clean-up trades than other sponsor-backed selldowns

Econometric models applied:

1. Ordinary least squares with sector-adjusted returns
2. Quantile regression with sector-adjusted returns
3. Ordinary least squares with country-adjusted returns
4. Quantile regression with country-adjusted returns

For this hypothesis we use an OLS model with first-day abnormal returns as the dependent variable, full sponsor exit as the independent variable, and a range of control variables. We estimate the model using a dataset that includes all sponsor-backed selldowns with discount data. The model is specified as follows for company i:

$$\begin{aligned} Return_{1d,i} =& \beta_0 + \beta_1 clean\text{-}up_i + \beta_2 percsold_i + \beta_3 \log selldown\_mcap_i + \beta_4 country_i \\ &+ \beta_5 industry_i + \beta_6 year_i + \beta_7 selldno_i + \epsilon_i \end{aligned} \tag{18}$$

Again, the binary variable *clean-up* is the independent variable of interest, meaning that a sponsor-backed selldown will have a β_1 effect on first-day returns. *percsold* is included to control for liquidity shocks that could potentially impact the price in the short-term aftermarket. Additional controls have been considered, but not included, due to either lack of theoretical evidence that it should be, or because no meaningful statistical relationship was found in determining the model. We find that homoscedasticity cannot be rejected. However, as the p-value from the White test is 0.1262 we rely on heteroscedasticity-consistent standard errors.

Running the OLS model with sector-adjusted returns, we get a β_1 estimate of 1.039, implying a 1.0 percentage points higher abnormal return on the first day after a clean-up trade as compared to a non-clean-up transaction. This relationship is statistically significant at the 1% alpha level. The economic significance of a more than 1 percentage point increase in abnormal performance over 1 day is substantial. Estimating the model with country-adjusted returns yields results of same magnitude and significance, as we would expect for a time period this short. For company i and quantile ρ in the QR model, we use the following specification:

$$\begin{aligned} Return_{1d,i} = & \beta_0^{\rho} + \beta_1^{\rho} \textit{clean-up}_i + \beta_2^{\rho} percsold_i + \beta_3^{\rho} \log selldown\_mcap_i + \beta_4^{\rho} country_i \\ & + \beta_5^{\rho} industry_i + \beta_6^{\rho} year_i + \beta_7^{\rho} selldno_i + \epsilon_i \end{aligned} \tag{19}$$

Table 11: Hypothesis 2b OLS and QR regressions. Dependent variable is first-day returns returns adjusted by country- and sector indexes

Variable	**OLS**	**OLS**	**QR (at 0.5)**	**QR (at 0.5)**
Error	Conv.	Conv.	Bootstr.	Bootstr.
Intercept	-11.17302	-12.39617	-9.28470	-12.34340
	(5.62954)***	(5.59581)***	(8.1173)	(6.0924)**
Clean-up	1.03853	0.98261	0.59170	0.72430
	(0.40160)***	(0.39919)***	(0.3773)	(0.3283)**
logselldown_mcap	0.56580	0.59126	0.40370	0.61570
	(0.19565)**	(0.19448)***	(0.1746)**	(0.1770)***
Perc_sold	-0.03177	-0.02804	-0.02170	-0.01440
	(0.02462)	(0.02447)	(0.0272)	(0.0210)
Net ret adj	Sector	Country	Sector	Country
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
Observations	721	721	721	721

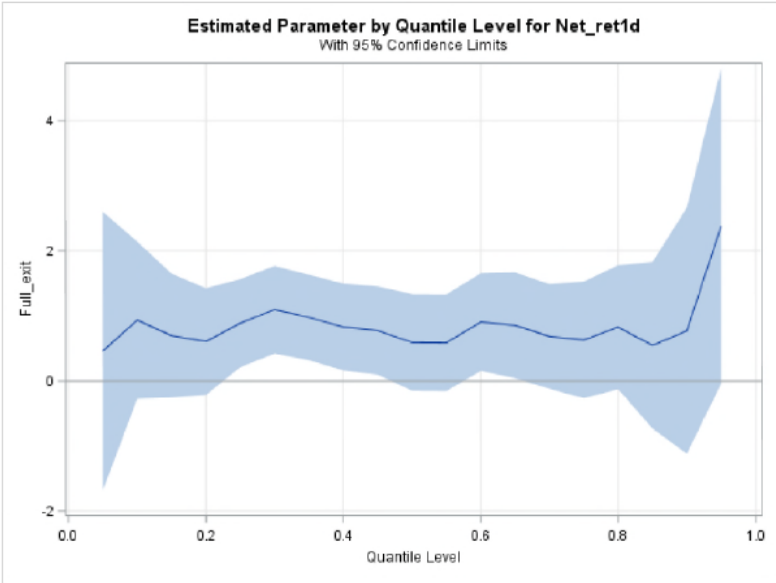

Figure 7: H2b QR regression with sector-adjusted returns

Estimating the model at the conditional median of the dependent variable, we get a coefficient estimate for *clean-up* of 0.5917. This estimate is statistically insignificant at the 10% alpha level with a p-value of 0.1169 using bootstrap standard errors. The Wald test gives a similar significance level at a p-value of 0.1164. As is evident from the quantile plot, the coefficient estimates have positive values for the entire distribution of returns although the estimates are insignificant at the 5% alpha level for most of it. The same can be said for the Wald test with 6 of the 19 quantiles being significant at the 5% level. When using country-adjusted returns, the QR model yields a positive point estimate of 0.7243 at the median, which is statistically significant at the 5% level. However, considering the effect across all quantiles, the overall significance is similar to the QR model using industry-adjusted returns.

As a robustness check, we fit the OLS model with returns over other time periods. Using returns for 1 week (p=0.1164), 2 weeks (p=0.1203), 1 month (p=0.3199) and 3 months (p=0.2419), we get no significant coefficient estimates for clean-up at the 10% alpha level. This implies that the effect is priced in on the first day of trading, rather than over a prolonged period and adds to the robustness of results. Market participants are more likely to be reacting only to the clean-up trade, when no effect is present over longer short-term periods.

In sum, the strongly positive and statistical significant relationship found in the OLS model is more nuanced when running the QR model. Although the estimates are positive for all quantiles, most are not significant at the 5% alpha level. Still, we deem the evidence of a positive relationship strong, given that estimates have the same direction and level across the entire distribution in the QR model, and is highly significant and positive in the OLS model.

7.2.3 Hypothesis 2c

H2c: Clean-up trades exhibit no 1-year abnormal returns after the selldown

Econometric models applied:

1. Ordinary least squares with sector-adjusted returns
2. Quantile regression with sector-adjusted returns
3. Ordinary least squares with country-adjusted returns
4. Quantile regression with country-adjusted returns

For testing this hypothesis, we use an OLS and a QR model. The dependent variable is sector index-adjusted 1-year returns, and the independent variable of interest is a binary variable indicating 1 if the transaction is a clean-up. In line with previous models we also fit the model with country index-adjusted returns as the dependent variable for robustness. The dataset used includes only sponsor transactions with discount data available.

The OLS model is specified as follows for company i:

$$\begin{aligned} Return_{1y,i} =& \beta_0 + \beta_1 clean\text{-}up_i + \beta_2 \log selldown\_mcap_i \\ &+ \beta_3 country_i + \beta_4 industry_i + \beta_5 year_i + \beta_6 selldno_i + \epsilon_i \end{aligned} \tag{20}$$

The coefficient estimate to *clean-up* corresponds to a $\beta_1 * 100\%$ change in adjusted 1-year return if the selldown is a full exit by pre-IPO sponsors. As in hypothesis 1b, we include only the control variables that are likely to impact expected returns. The White test gives us a p-value of 0.1625 meaning that we cannot reject the null that the error term is homoscedastic. However, as the p-value is fairly low we report the heteroscedasticity-consistent errors.

The coefficient estimate for clean-up in the OLS model is -7.2897 and is highly significant with a p-value 0.0253. This result is robust to the inclusion of control variables for performance and time since the IPO (not reported). Further, the coefficient estimate to clean-up is of similar economic and statistical significance if fitting the model with country index-adjusted returns as the dependent variable.

We specify the QR model for company i at the ρth quantile as below:

$$\begin{aligned} Return_{1y,i} =& \beta_0^\rho + \beta_1^\rho clean\text{-}up_i + \beta_2^\rho \log selldown\_mcap_i + \beta_3^\rho country_i \\ &+ \beta_4^\rho industry_i + \beta_5^\rho year_i + \beta_6^\rho selldno_i + \epsilon_i \end{aligned} \tag{21}$$

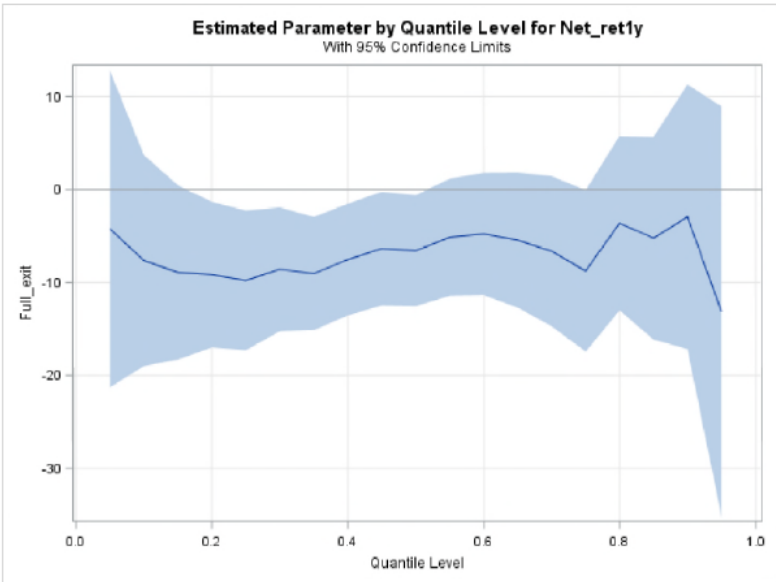

Figure 8: H2c QR regression with sector-adjusted returns

Fitting the QR model, we get negative coefficient estimates for *clean-up* across all 19 quantiles. The significance of these estimates as measured by the bootstrap errors varies over the distribution. Generally, for the median and quantiles below, the coefficient estimate is significant at the 5% alpha level. The median estimate is slightly less negative than in the OLS model at -6.5768, and the significance level

Table 12: Hypothesis 2c OLS and QR regressions. Dependent variable is 1-year returns adjusted by country- and sector indexes

Variable	**OLS**	**OLS**	**QR (at 0.5)**	**QR (at 0.5)**
Error	White	White	Bootstrap	Bootstrap
Intercept	-27.49056	-47.54639	-31.19360	-37.01720
	(38.31812)	(35.50355)	(53.5841)	(62.8332)
Clean-up	-7.28969	-7.84002	-6.57680	-6.78180
	(3.2508)**	(3.32182)**	(3.0371)**	(3.1285)**
logselldown_mcap	-0.90140	-0.52115	0.02770	0.90270
	(1.87387)	(1.88611)	(1.3745)	(1.3669)
Net ret adj	Sector	Country	Sector	Country
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown no. control	Yes	Yes	Yes	Yes
Observations	721	721	721	721

is slightly weaker at a p-value of 0.0352. Running the Wald test, we get significant coefficient estimates for the 0.15th through 0.50th quantile at a 5% alpha level. In sum, the QR model results are more ambiguous than in the OLS model.

By observing the entire distribution, we get a better picture of the effect of clean-up trades on 1-year returns. Although coefficient estimates are not statistically significant across all estimated quantiles, we are fairly confident in the results showing a negative impact from clean-up trades. This confidence is mostly attributed to all estimates being well below zero and the median estimate being significant at the 5% alpha level. Again, the results are robust to country index-adjusted returns as the dependent variable with no significant deviations with respect to economic and statistical significance.

The QR model largely supports the results from the OLS showing a strong negative impact of clean-up trades on 1-year abnormal returns. This confirms our hypothesis that when the sponsor is no longer a part of the company, the positive abnormal performance of sponsor-backing previously found reverts to a negative effect. Whether this is attributable to weaker operating performance, more negative perception of the future without a large shareholder, or simply regression to the mean, is unclear.

7.3 Hypothesis group 3: Lock-up provision effects

7.3.1 Hypothesis 3a

H3a: Sponsors are more likely to do selldowns sooner after lock-up expiry than non-sponsors

Econometric models applied:

1. Ordinary least squares
2. Ordinary least squares with momentum control
3. Probit
4. Probit with momentum control

We use two separate models for testing this hypothesis. The main difference between the two is the dependent variable, which for the OLS is log*lockupdays* (the natural logarithm of days from expiry of lock-up to the pricing date of the selldown), and for the probit is *instantselldown* (binary variable taking on the value 1 if selldown happens within 5 trading days from lock-up expiry and 0 otherwise). Both are included as the former is a general test for what type of owner takes the longest to do selldowns, while the latter estimates who is most likely to do selldowns shortly after lock-up expiry. Including both adds to the robustness of the results. We run both models using the dataset with all selldowns that have lock-up provisions. The first OLS model is specified as follows for company i:

$$\begin{aligned} \log lockupdays_i = &\beta_0 + \beta_1 selldown\_spons_i + \beta_2 \log selldown\_mcap_i + \beta_3 country_i \\ &+ \beta_4 industry_i + \beta_5 year_i + \beta_6 selldno_i + \epsilon_i \end{aligned} \tag{22}$$

The OLS model with momentum control is specified as follows:

$$\begin{aligned}\log lockupdays_i = &\beta_0 + \beta_1 selldown\_spons_i + \beta_2 \log selldown\_mcap_i + \beta_3 country_i \\ &+ \beta_4 industry_i + \beta_5 year_i + \beta_6 selldno_i + \beta_7 changetoipo_i + \epsilon_i\end{aligned} \tag{23}$$

We do not include control variables other than the five generic controls. None of the other controls have proven to be meaningful to include in this model. This is mainly due to the fact that we are simply testing for the time between lock-up expiry and selldown. Some other unobserved company-specific factors might influence this other than sponsor-backing, but it is argued that this is at least partially captured through the generic controls. The White test gives a p-value of 0.3116, implying that we cannot reject homoscedasticity.

The coefficient estimate to *selldown_spons* is -1.1266 and is highly significant with a p-value of less than 0.0001. As the model is estimated as linear-level, the untransformed dependent variable is multiplied by e^{β} for a 1 unit change in the independent variable. The beta estimate thus implies that sponsors-backing results in an approximate 67.5% reduction of days from lock-up expiry to selldown, holding other factors constant. Including momentum control in the model does not shift the results in any meaningful way. The coefficient estimate to *selldown_spons* is -1.0831 in this model and still significant with a p-value of less than 0.0001. The implied change in days from lock-up expiry to selldown is a 66.1% reduction. Thus, when controlling for momentum effects, the effect of sponsor-backing is marginally smaller, but the estimate is not different from the first at any reasonable level of significance.

To get a better understanding of the distribution across observations of time from lock-up expiry to selldown for sponsors and non-sponsors, we estimate a probit model with *instantselldown* as the dependent variable. We use the same controls here as in the linear model above. The model is specified as below and estimated by maximum likelihood:

$$P(instantselldown = 1 \mid selldown\_spons_i, \log selldown\_mcap_i, country_i, industry_i, year_i, selldno_i) = \phi(z) = \phi(\beta_0 + \beta_1 selldown\_spons_i + \beta_2 \log selldown\_mcap_i + \beta_3 country + \beta_4 industry_i + \beta_5 year_i + \beta_6 selldno_i) \tag{24}$$

And similarly with momentum control:

$$P(instantselldown = 1 \mid selldown\_spons_i, \log selldown\_mcap_i, country_i, industry_i, year_i, selldno_i, changetoipo_i) = \phi(z) = \phi(\beta_0 + \beta_1 selldown\_spons_i + \beta_2 \log selldown\_mcap_i + \beta_3 country + \beta_4 industry_i + \beta_5 year_i + \beta_6 selldno_i + \beta_7 changetoipo_i) \tag{25}$$

The coefficient to *selldown_spons*, β_1, is the change in z-value following a 1 unit change in the variable, holding all control variables constant. This allows us to estimate the marginal effect of sponsor-backing on the probability of doing a selldown within 5 trading days from lock-up expiry. Measuring the marginal effect of a given increase in share price following the IPO is more complicated when multiple control variables are included in the model. The beta estimate for *selldown_spons* is a partial derivative, leading it to take on different values depending on the value of the remaining variables in the model. Thus, calculating the marginal effect for company i being sponsor-backed can be done as follows (Stock and Watson, 2012):

$$\frac{\partial P(Y_i = 1 \mid X_{1i}, \ldots, X_{Ki}; \beta_0, \ldots, \beta_K)}{\partial X_{ki}} = \beta_k \phi(\beta_0 + \sum_{k=1}^{K} \beta_k X_{ki}) \tag{26}$$

Where $\phi(\cdot)$ is the cumulative standard normal distribution. As we have multiple independent regressors, the marginal effect of sponsor-backing will dependent on

Table 13: Hypothesis 3a OLS and Probit regressions. Dependent variable is log*lockupdays* for OLS and *instant_selldown* for Probit

Variable	**OLS**	**OLS**	**Probit**	**Probit**
Error	Conventional	Conventional	Conventional	Conventional
Intercept	6.57704 (2.48231)***	6.24362 (2.48868)***	-13.26360 (211.7)	-13.37670 (212.1)
Selldown_Spons	-1.12659 (0.17320)***	-1.08305 (0.17517)***	0.54130 (0.1771)***	0.55410 (0.1792)***
logselldown_mcap	-0.23267 (0.07208)***	-0.24750 (0.07261)***	0.09170 (0.0714)	0.09000 (0.0717)
ChangetoIPO		0.000853 (0.000535)		0.00027 (0.00061)
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
Observations	857	857	857	857

the value of each of these. We estimate the marginal effect of sponsor-backing at the mean, and use the model without the momentum control as it has shown no substantial effect. Specifically, we run the QLIM procedure in SAS to estimate the marginal effect of the covariate at each observation. Following the approach of Greene (2003), these marginal effects are then averaged over the full sample to get the average marginal effect on the dependent variable from a 1 unit change in the independent variable. We get an estimated effect of a 9.0% increase in the probability of an owner doing a selldown within 5 trading days of lock-up expiry if the owner is a sponsor. The average probability of selling down instantly is approximately 13% in the sample, making a 9% increase substantial. The results are of equal magnitude and significance when including the momentum control variable.

7.3.2 Hypothesis 3b

H3b: The likelihood of non-sponsors doing a selldown shortly after lock-up expiry is positively related to increase in valuation since the IPO

Econometric models applied:

1. Probit

For testing this hypothesis, we use a dataset consisting only of non-sponsor-backed selldowns with lock-up provisions. We test the hypothesis using *instantselldown* as the dependent variable. We employ a probit model as we have a binary variable as the dependent variable. We use *changetoipo* as the independent variable and specify the model as follows:

$$\begin{aligned} P(instantselldown = &1 \mid changetoipo_i, \log selldown\_mcap_i, \log daysipo_i, country_i, industry_i, \\ &year_i, selldno_i) = \phi(z) = \phi(\beta_0 + \beta_1 changetoipo_i + \beta_2 \log selldown\_mcap_i \\ &+ \beta_3 \log daysipo_i + \beta_4 country_i + \beta_5 industry_i + \beta_6 year_i + \beta_7 selldno_i) \end{aligned} \tag{27}$$

Table 14: Hypothesis 3b Probit regression. Dependent variable is *instant_ selldown*

Variable	**Probit**
Error	Conventional
Intercept	2.15020 (76.3488)
ChangetoIPO	0.01710 (0.00787)**
logselldown_mcap	-0.85420 (0.5278)
logdaysIPO	-2.36070 (0.9565)**
Country dummy	Yes
Sector dummy	Yes
Year dummy	Yes
Selldown no. control	Yes
Observations	277

In addition to the generic control variables, we control for number of days since the IPO (log*daysIPO)*. As stock price is an increasing function of time, number of days since the IPO is most likely correlated with returns since the IPO. Thus, we control for the effect of being listed for a longer period to isolate the effect of returns since the IPO on the probability of doing an instant selldown. Logarithmic transformation of the variable has been used to reduce the weight of outliers.

The coefficient β_1 is estimated at 0.0171, which is not directly interpretable. The estimate is significant at the 5% alpha level using the Wald test. The marginal effect of returns from the IPO on probability of doing a selldown within 5 trading days from lock-up expiry can be calculated as in model two in hypothesis 3a. We use the QLIM procedure as computed by SAS and average the marginal effect across all observations. This yields an average marginal effect of $7.84e^{-4}$, implying a 7.84% increase in probability of doing an instant selldown if the market capitalisation has increased by 100% since the IPO, holding all else equal. In the sample, 84 out of 277 non-sponsored selldowns with lock-up provisions saw market capitalisation increase 100% or more between IPO and selldown with the average increase in market cap being 98% and the median 50%. Therefore, we consider the effect statistically significant and the economic impact material.

7.3.3 Hypothesis 3c

H3c: The short-run market reaction in terms of discount and first-day returns will be relatively more negative when a non-sponsor does a selldown shortly after lock-up expiry as compared to other non-sponsored selldowns

Econometric models applied:

1. OLS with discount as dependent variable
2. QR with discount as dependent variable
3. OLS with first-day sector-adjusted returns as dependent variable
4. QR with first-day sector-adjusted returns as dependent variable
5. OLS with first-day country-adjusted returns as dependent variable
6. QR with first-day country-adjusted returns as dependent variable

In testing this hypothesis, we use a dataset consisting of non-sponsored selldowns with lock-up provisions and discount data. As mentioned, we test for effects on discounts and first-day returns, respectively. First, we test for effects on discount, running the models with *discount* as dependent variable and *instantselldown* as the independent variable of interest. We include the same controls as in other models where discount is the dependent variable. The OLS model is specified as follows for company i:

$$\begin{aligned} Discount_i = &\beta_0 + \beta_1 instantselldown_i + \beta_2 percsold_i + \beta_3 changetoipo_i \\ &+ \beta_4 \log syndicateint_i + \beta_5 \log selldown\_mcap_i + \beta_6 country_i \\ &+ \beta_7 industry_i + \beta_8 year_i + \beta_9 selldno_i + \epsilon_i \end{aligned} \tag{28}$$

We cannot reject the null that errors are homoscedastic, and therefore report conventional standard errors. The coefficient estimate β_1 is 0.2520 with a corresponding p-value of 0.7542. Thus, the OLS estimator indicates no statistically significant relationship between discounts and *instantselldown* for non-sponsored selldowns, which goes against the hypothesis of a negative relationship. To explore the relationship further, we run a quantile regression using the same variables. The model is specified as follows for company i at the ρth quantile:

$$\begin{aligned} Discount_i = &\beta_0^\rho + \beta_1^\rho instantselldown_i + \beta_2^\rho percsold_i + \beta_3^\rho changetoipo_i \\ &+ \beta_4^\rho \log syndicateint_i + \beta_5^\rho \log selldown\_mcap_i + \beta_6^\rho country_i \\ &+ \beta_7^\rho industry_i + \beta_8^\rho year_i + \beta_9^\rho selldno_i + \epsilon_i \end{aligned} \tag{29}$$

The coefficient estimate at the median is -0.5123, but this is statistically insignificant with a p-value of 0.5952 using bootstrap standard errors. The Wald test show similar significance levels. The estimates turn from negative to positive at different quantiles and are generally insignificant across all quantiles for all test statistics. Taken in extension of the OLS estimator, the models, surprisingly, gives strong evidence that there exists no statistical relationship between discounts and instant selldowns for non-sponsored transactions.

We test for a potential effect of doing an instant selldown on first-day returns using similar OLS and QR specification. For this test, we use the same controls as for other models with first-day returns as the dependent variable. We specify the OLS model for company i as follows:

Table 15: Hypothesis 3c OLS and QR regressions. Dependent variable is selldown discount

Variable	**OLS**	**QR (at 0.5)**
Error	Conventional	Bootstrap
Intercept	-5.76399 (5.91205)	-4.6060 (7.2797)
Instant_selldown	0.25201 (0.80373)	-0.5123 (0.8251)
logABB_mcap	1.23780 (0.28634)***	1.0048 (0.2948)***
logdaysipo	-0.50839 (0.30595)*	-0.4642 (0.2429)*
logsyndicateint	-0.83008 (0.28187)***	-0.6068 (0.2637)**
ChangetoIPO	-0.00004043 (0.00136)	0.0002 (0.0013)
Country control	Yes	Yes
Industry control	Yes	Yes
Year dummy	Yes	Yes
Selldown # control	Yes	Yes
Observations	262	262

$$\begin{aligned} Return_{1d,i} =& \beta_0 + \beta_1 instantselldown_i + \beta_2 percsold_i + \beta_3 \log selldown\_mcap_i \\ &+ \beta_4 country_i + \beta_5 industry_i + \beta_6 year_i + \beta_7 selldno_i + \epsilon_i \end{aligned} \tag{30}$$

Using the White test, we cannot reject the hypothesis of homoscedasticity for any level of significance. Thus, we use conventional standard errors in the OLS model.

The β_1 estimate is 1.0427 with a p-value of 0.3302 when fitting the model with sector index-adjusted returns. Again, we find a positive but statistically insignificant relationship between the market reaction and an instant selldown by a non-sponsor. Country-index adjusted returns yield almost identical results as expected. We run the QR model using the same variable as in the OLS model and specify as follows

Table 16: Hypothesis 3c OLS and QR regressions. Dependent variable is first-day returns

Variable	**OLS**	**OLS**	**QR (at 0.5)**	**QR (at 0.5)**
Intercept	-3.58645	-2.29508	-3.7755	-3.8042
	(7.62779)	(7.58295)	(8.5806)	(10.9625)
Instant_selldown	1.04265	1.11928	1.0413	1.0874
	(1.06797)	(1.06170)	(1.2682)	(1.3345)
logABB_mcap	0.18499	0.09988	0.2687	0.2430
	(0.30887)	(0.30706)	(0.2688)	(0.3185)
Perc_sold	-0.02867	-0.03530	-0.0304	-0.0383
	(0.03968)	(0.03945)	(0.0336)	(0.0392)
Net ret adj	Sector	Country	Sector	Country
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
Observations	262	262	262	262

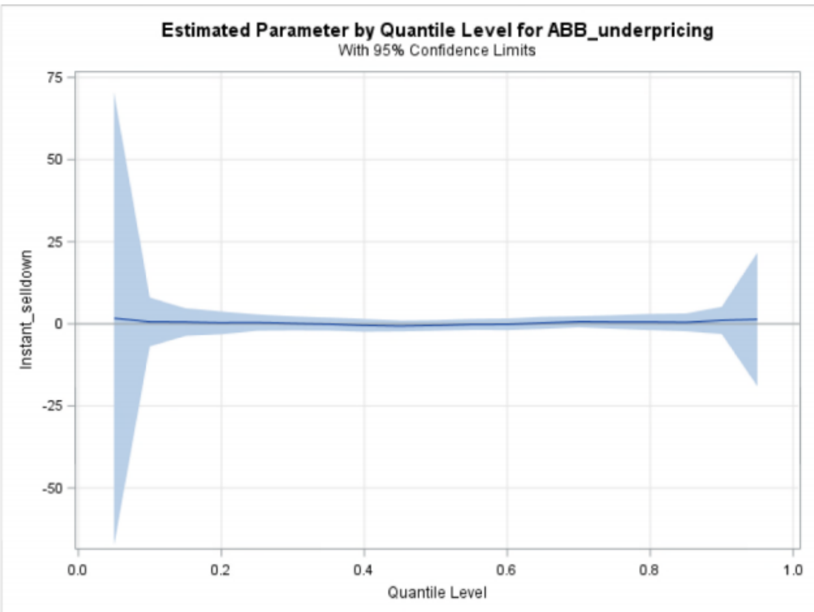

Figure 9: H3c QR regression with discount as dependent variable

for company i at quantile ρ:

$$\begin{aligned} Return_{1d,i} = & \beta_0^\rho + \beta_1^\rho instantselldown_i + \beta_2^\rho percsold_i + \beta_3^\rho \log selldown\_mcap_i \\ & + \beta_4^\rho country_i + \beta_5^\rho industry_i + \beta_6^\rho year_i + \beta_7^\rho selldno_i + \epsilon_i \end{aligned} \quad (31)$$

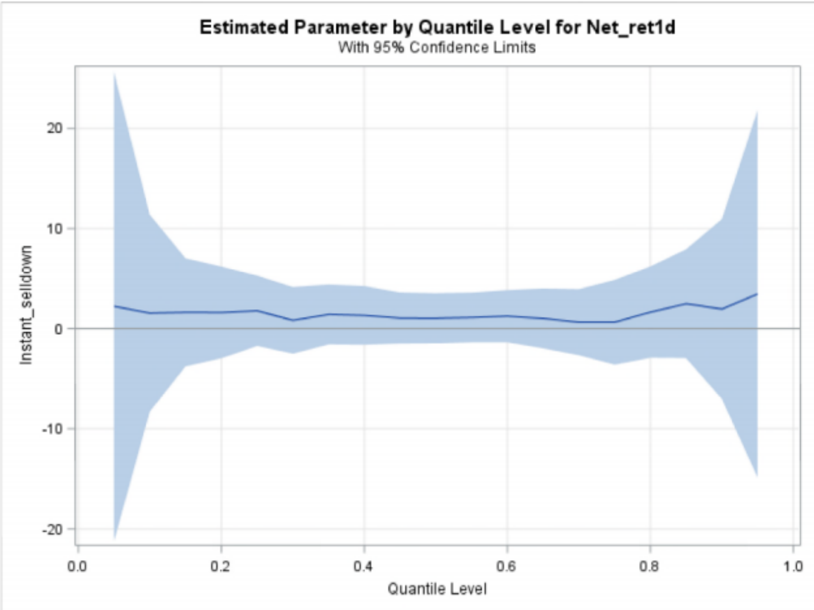

Figure 10: H3c QR regression with sector-adjusted one-day returns as dependent variable

The coefficient estimate to *instantselldown* at the median is 1.0413 and insignificant using bootstrap standard errors and the Wald test. The estimates are above zero for all quantiles, although they remain insignificant across the entire distribution when using bootstrap standard errors and the Wald test. In sum, the evidence that first-day abnormal returns are not significantly different from zero in an instant selldown by a non-sponsor are substantial.

Testing for effects of an instant selldown on both discounts and first-day returns for non-sponsors, we find no evidence of a statistical relationship. The models applied for testing this hypothesis provide strong evidence, however, that no such relationship exists. This goes against the reasoning that laid the ground for this hypothesis, meaning that other dynamics apply to selldowns in this case. Either our arguments are flawed when we assess investor behaviour around these selldowns, or the results suffer from omitted variable bias or causal ambiguity. In any case, we find no evidence supporting this hypothesis.

7.3.4 Hypothesis 3d

H3d: The likelihood of sponsors doing a selldown shortly after lock-up expiry is independent of the increase in market valuation since the IPO

Econometric models applied:

1. Probit

For modelling purposes, the same approach as in hypothesis 3b is applied. We use a probit model with the same dependent-, independent- and control variables:

$$\begin{aligned}P(instantselldown = 1 \mid\, & changetoipo_i, \log selldown\_mcap_i, \log daysipo_i, country_i, industry_i,\\ & year_i, selldno_i) = \phi(z) = \phi(\beta_0 + \beta_1 changetoipo_i + \beta_2 \log selldown\_mcap_i\\ & + \beta_3 \log daysipo_i + \beta_4 country_i + \beta_5 industry_i + \beta_6 year_i + \beta_7 selldno_i)\end{aligned} \tag{32}$$

The only change from hypothesis 3b is that we run the model using a dataset of only sponsor-backed selldowns with lock-up provisions. The coefficient estimate to *changetoipo* is $3.4e^{-4}$ in this model with a p-value from the Wald test of 0.7626. Thus, the estimate is insignificant for any reasonable alpha level. The average marginal effect is $5.86e^{-5}$, implying a 0.6% increase in the probability of *instantselldown* for a 100% increase in market capitalisation since the IPO. Besides being insignificant, the implied effect on probability is also negligible. This provides evidence that sponsors do not put as much weight on valuation, as we hypothesised. Taken in context with hypotheses 3a and 3c, there is strong evidence that sponsors are more likely to do an instant selldown, regardless of realised returns since the IPO, where non-sponsors are more likely to sell shortly after lock-up expiry if valuation has gone up significantly.

Table 17: Hypothesis 3d Probit regression. Dependent variable is instant_selldown

Variable	**Probit**
Error	Conventional
Intercept	-5.91470 (359.1)
ChangetoIPO	0.00034 (0.00113)
logselldown_mcap	-0.02060 (0.1147)
logdaysIPO	-1.18300 (0.1968)***
Country control	Yes
Industry control	Yes
Year control	Yes
Selldown # control	Yes
Observations	580

7.3.5 Hypothesis 3e

H3e: The short-run market reaction in terms of discount and first-day returns will not be significantly different when a sponsor does a selldown shortly after lock-up expiry as compared to other sponsor-backed selldowns

Econometric models applied:

1. OLS with discount as dependent variable
2. QR with discount as dependent variable
3. OLS with first-day sector-adjusted returns as dependent variable
4. QR with first-day sector-adjusted returns as dependent variable
5. OLS with first-day country-adjusted returns as dependent variable
6. QR with first-day country-adjusted returns as dependent variable

The methodology for testing this hypothesis is the same as in H3c. We specify the same models using the same variables. In addition, we include *clean-up* to control for clean-up trade effects. The data set used for estimation includes all sponsor-backed selldowns with discount data and lock-up provisions. As in H3c, we cannot reject the null hypothesis that errors are homoscedastic when running the White test. We specify the OLS model for company i as follows:

$$\begin{aligned} Discount_i =& \beta_0 + \beta_1 instantselldown_i + \beta_2 percsold_i + \beta_3 changetoipo_i \\ & + \beta_4 \log daysipo_i + \beta_6 \log selldown\_mcap_i + \beta_7 country_i + \beta_8 industry_i \\ & + \beta_9 year_i + \beta_{10} selldno_i + \beta_{11} clean\text{-}up_i + \epsilon_i \end{aligned} \tag{33}$$

The coefficient to *instantselldown* is estimated at -0.5377, and, as with non-sponsored selldowns, is statistically insignificant. This is in line with the hypothesis of a sponsor-backed instant selldown having no significant impact on the size of the discount. To see if this is also the case at the median and other quantiles of the distribution, we run a QR model with the same variables. The QR model is specified as follows for company i at the ρth quantile:

$$\begin{aligned} Discount_i =& \beta_0^\rho + \beta_1^\rho instantselldown_i + \beta_2^\rho percsold_i + \beta_3^\rho changetoipo_i \\ & + \beta_4^\rho \log daysipo_i + \beta_6^\rho \log selldown\_mcap_i + \beta_7^\rho country_i \\ & + \beta_8^\rho industry_i + \beta_9^\rho year_i + \beta_{10}^\rho selldno_i + \beta_{11}^\rho clean\text{-}up_i + \epsilon_i \end{aligned} \tag{34}$$

The coefficient estimate to *instantselldown* at the median is -0.0829. This estimate is insignificant with a p-value of 0.7824 using bootstrap standard errors. The estimate β_1^ρ takes on both negative and positive values depending on the quantile and remains insignificant across the full distribution of the dependent variable. The Wald test also gives insignificant test statistics throughout the quantiles. Thus, the OLS and QR models both provides strong evidence that there is no significant relationship between discount and instant selldowns for sponsor-backed transactions. This supports the hypothesis with regards to discounts.

We test for effects on first-day excess returns as in H3c. We run the same models,

Table 18: Hypothesis 3e OLS and QR regressions. Dependent variable is selldown discount

Variable	**OLS**	**QR (at 0.5)**
Error	Conv.	Bootstr.
Intercept	-8.64525 (3.53026)**	-8.2535 (3.1567)***
Instant_selldown	-0.53772 (0.37935)	-0.0829 (0.2999)
logABB_mcap	1.33243 (0.19945)***	1.1594 (0.1400)***
logdaysipo	-0.42114 (0.21112)**	-0.2100 (0.1622)
logsyndicateint	-0.50276 (0.17619)***	-0.4452 (0.1367)***
ChangetoIPO	0.00036654 (0.00123)	0.0003 (0.0012)
Country control	Yes	Yes
Industry control	Yes	Yes
Year control	Yes	Yes
Selldown # control	Yes	Yes
Observations	562	562

changing only the data set used for estimation to contain sponsor-backed selldowns rather than non-sponsored. The OLS model for company i can be specified as follows:

$$\begin{aligned} Return_{1d,i} =& \beta_0 + \beta_1 instantselldown_i + \beta_2 percsold_i + \beta_3 \log selldown\_mcap_i \\ &+ \beta_4 country_i + \beta_5 industry_i + \beta_6 year_i + \beta_7 selldno_i + \beta_8 clean\text{-}up_i + \epsilon_i \end{aligned} \tag{35}$$

The White test cannot reject homoscedasticity for this model. Using sector index-adjusted returns, we get a coefficient estimate of -0.6364. However, this estimate is statistically insignificant at any reasonable alpha level. Again, this is in accordance

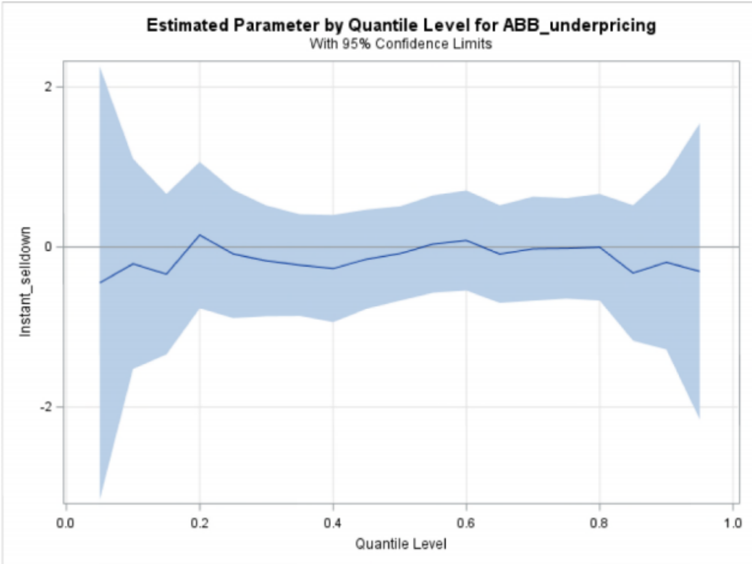

Figure 11: H3e QR regression with discount as dependent variable

with the hypothesis of no effect. The results using country index-adjusted returns do not deviate from the above in any significant way.

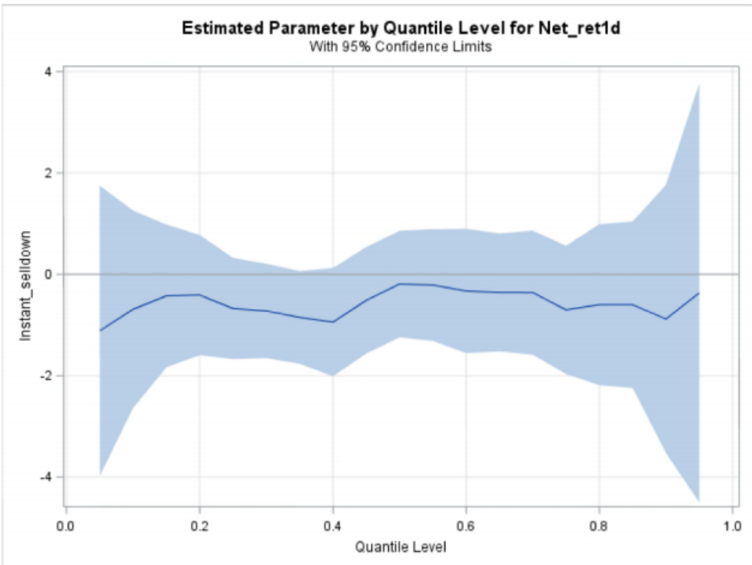

Figure 12: H3e QR regression with sector-adjusted one-day returns as dependent variable

To see if this holds using alternative model specifications, we estimate the QR model using the same variables as above. We specify the QR model for company i at the

Table 19: Hypothesis 3e OLS and QR regressions. Dependent variable is first-day returns

Variable	**OLS**	**OLS**	**QR (at 0.5)**	**QR (at 0.5)**
Error	Conv.	Conv.	Bootstr.	Bootstr.
Intercept	-8.25464 (4.59162)*	-8.25464 (4.59162)*	-10.0896 (4.8375)**	-10.0896 (4.8204)**
Instant_selldown	-0.63642 (0.54478)	-0.63642 (0.54478)	-0.1924 (0.5344)	-0.1924 (0.5120)
logABB_mcap	0.75113 (0.22369)***	0.75113 (0.22369)***	0.7126 (0.2038)	0.7126 (0.1851)***
Perc_sold	0.00403 (0.02815)	0.00403 (0.02815)	0.0090 (0.0296)	0.0090 (0.0273)
Net ret adj	Sector	Country	Sector	Country
Country control	Yes	Yes	Yes	Yes
Industry control	Yes	Yes	Yes	Yes
Year control	Yes	Yes	Yes	Yes
Selldown # control	Yes	Yes	Yes	Yes
Observations	562	562	562	562

ρth quantile as follows:

$$\begin{aligned} Return_{1d,i} =& \beta_0^\rho + \beta_1^\rho instantselldown_i + \beta_2^\rho percsold_i + \beta_3^\rho changetoipo_i \\ &+ \beta_4^\rho \log syndicateint_i + \beta_5^\rho \log selldown\_mcap_i + \beta_6^\rho country_i \\ &+ \beta_7^\rho industry_i + \beta_8^\rho year_i + \beta_9^\rho selldno_i + \beta_{10}^\rho clean\text{-}up_i + \epsilon_i \end{aligned} \tag{36}$$

The median coefficient estimate for *instantselldown* is -0.1924. As for discounts, the estimate is statistically insignificant for all three test statistics. Although estimates remain negative for all quantiles, they are insignificant at the 5% alpha level using the bootstrap standard errors and the Wald test. Country index-adjusted returns yield the same inference. In sum, we find no relationship between discounts or first-day returns and instant selldowns for sponsor-backed transactions. This finding is robust to both return adjustment procedures, and to different model specifications. We consider the evidence in favour of the hypothesis as being strong.

8 Discussion

8.1 Effect of sponsor-backing on discounts and returns

Theory suggests that sponsor-backed IPOs have smaller underpricing and better long-term aftermarket performance than non-sponsored IPOs. One of the most compelling explanations why sponsors are able to obtain a higher price and, correspondingly, smaller underpricing is the certification effect, describing that sponsors rely on public markets as an exit option in the future. This should incentivise them not to overcharge in any one single IPO, but rather to consider reputational effects on future equity markets transactions. Broadly, our findings suggest that the same is true for selldowns. We estimate financial sponsors reducing discounts between 0.3 percentage points and 0.6 percentage points relative to non-sponsors, which is highly economically significant given previously found average SEO discounts in the 3% range (Bortolotti et al., 2008).

Share overhang and prospect theories also led us to formulate hypotheses regarding clean-up trades. These hypotheses state that when the post-selldown stake is zero, the sponsor will be inclined to maximise price, thereby minimising discount. If we control for this effect, the magnitude of the effect of being sponsor-backed decreases and falls just short of being statistically significant at the 5% alpha level. This indicates that it is possible that the certification effect exists, but the models also reveal that a large part of the effect of sponsor-backing on discount is only realised in the clean-up trade. Thus, it seems that sponsor-backing does indeed have a positive effect on discounts, but this effect is somewhat offset by the negative share overhang effect, until the clean-up trade eliminates the overhang.

Likewise, we find support for the extensively covered empirical finding that sponsor-backed IPOs deliver positive abnormal returns in the aftermarket. The models find economically and statistically significant overperformance, with point estimates in the 6-8% range, which is in line with the IPO literature. Including the clean-up trade control variable, we find stronger overperformance from being sponsor-backed, while the clean-up effect is negative and weakly significant. We get an indication that the abnormal performance decreases after the financial sponsor exits fully. In sum, this supports our hypothesis as well as existing literature: sponsor-backed companies have positive abnormal performance one year after the selldown, but the effect decreases when the sponsor is no longer invested.

8.2 Clean-up trades

The first two hypotheses on clean-up trades were generated based on previous literature presenting evidence that share overhang has a negative price impact. We hypothesise that when the post-selldown overhang is zero, the discount should be significantly smaller than for other selldowns. We get a highly significant result, with point estimates suggesting that clean-ups have about one quarter smaller discounts compared to other sponsor-backed selldowns.

There are two reasons why we would expect these results. First, the common reasons for discounts – prospect theory and information momentum effect – no longer hold for the clean-up trade as the post-selldown holdings of the financial sponsor is zero, which would incentivise the financial sponsor to get the smallest possible discount for the clean-up trade. Second, investors are likely to accept buying at the lower discount because the threat of further selldowns is removed. Third, investors will have the last chance of increasing its stake in the given company significantly without pushing up the price.

If we accept the premise that investors in selldowns consider clean-up trades preferable to other selldowns, as indicated by their willingness to accept smaller discounts, we would expect non-selldown investors to have a similar attitude when news of the clean-up becomes public information. This is exactly what we find, with the clean-up effect having economically meaningful, statistically significant positive impact on first-day returns, as compared to non-clean-up selldowns. While still negative, our results show a positive impact of approximately 0.6-1.0 percentage points depending on the estimated model. This is a material difference when mean first-day returns of this sample is approximately -2.6%. For robustness, we ran the same model over longer (but still short-run) time periods. We find that the effect is priced out already in the first day after the selldown with no significant effects for other time periods. This supports the notion that we have in fact isolated the clean-up effect.

As indicated by the results in hypothesis 1b, we identify worse abnormal returns on a 1-year basis when a sponsor fully exits an investment, compared to companies that remains sponsor-backed. The estimated magnitude of the effect in hypothesis 2c is highly economically significant, as the clean-up removes the positive abnormal returns of the previously sponsor-backed company. It is slightly surprising that the clean-up effect is evident already within the first year, as many of the alleged benefits of the private equity model should persist longer, e.g. skilled management and better overall operational performance. This could be attributable to the selldown taking place some time from the IPO, where the sponsor is likely to have relinquished control in many ways.

There are three possible explanations for the longer-term underperformance. One is that the overperformance of sponsor-backed IPO and SEO companies is explained by agency benefits of the private equity model, and those effects cease when the sponsor exits in the clean-up trade. The other possibility is that sponsors are better at timing their exits. However, seeing as the IPO and selldown process takes place over years, it would be extremely difficult for financial sponsors to perfectly time their clean-up trade relative to equity market sentiment. Third, it is possible that sponsors manage earnings. So-called "window dressing" refers to the practice of manipulating accounting performance so the company appears more attractive when it is time for the owner to sell. Once the sponsor has exited the investment fully, performance may revert as financial reporting is no longer subject to window dressing. Again, the prolonged exit process through selldowns makes this unlikely to have a substantial effect on average.

8.3 Lock-up

For this hypothesis group, We begin by testing a fundamental premise for the main lock-up based hypotheses: financial sponsors are incentivised to sell down sooner than non-sponsors. We find that non-sponsors, on average, take more than twice as long to sell down after lock-up expiry compared to financial sponsor sellers. We also see that sponsors are, on average, about 9 percentage points more likely than non-sponsors to sell down within five trading days of lock-up expiry (what we define as an immediate selldown). This is what we would expect given the incentive structure that sponsors face, where they are rewarded for returning money to investors sooner and have a predetermined maturity for the fund. On the other hand, we would expect non-sponsors to care more about a simple money-back multiple, as they do not face the same payoff function as sponsors.

Examining the consequences of this incentive structure further, we find that non-sponsors are nearly 8% more likely to sell down immediately after expiry of the lock-up when the value of the share has doubled since the IPO. As expected, the same is not true for our sample of sponsor-backed transactions, where we find no statistically significant relationship between the returns since IPO and the likelihood of selling immediately after lock-up expiry. Taken together, we deem evidence strong that non-sponsors are more likely to sell down in a company when it has delivered high returns.

Finding significant differences in the factors relating to timing of selldowns, we test if markets react differently to the decision to do a selldown immediately after expiry for sponsor-backed and non-sponsored companies, respectively. The rationale is that if non-sponsors sell down immediately after lock-up expiry, signaling that valuation is high, markets should react negatively. This will result in larger discount and worse first-day performance. Despite indications that non-sponsors are more aggressive with respect to valuation, none of the models provide evidence that discount and first-day returns on immediate selldowns are different for sponsor-backed and non-sponsored transactions. We suspect that the reason for the non-significant results is that the models do not take into account whether the selldown announcement comes as a surprise to markets or not. It is likely that markets will already have an idea as to whether a given owner is likely to do the selldown shortly after lock-up expiry, or if the holding period will be longer. A more thorough analysis of the company and owners is needed in testing this notion, but it is reasonable to expect this information to be available to professional investors. If that is the case, then markets would have already priced in the selldown before the announcement, which would remove the effect we are striving to estimate. In order to overcome this potential shortcoming, we would have to identify *surpise* immediate selldown and isolate the effect these have on discounts and first-day returns. This would require a subjective classification of each transaction in the dataset. Due to data limitations, we were unable to incorporate the surprise announcement factor, which left us with inconclusive evidence on hypotheses 3c and 3e.

8.4 Validity and reliability of results

The methodology used in this paper is not without flaws. It is possible that data quality has affected result in certain aspects, thus reducing the reliability of results. In addition to potential issues arising from the methodological choices taken, it is possible that our results are subject to omitted variable bias, meaning that some unobserved factor is causing the variation in both the dependent and independent variables of interest. The former would reduce the reliability of results, while the latter would impact the validity. The following sections will discuss possible causal ambiguities and data quality issues of the results obtained.

8.4.1 Omitted variable biases and casual ambiguities

The validity of results is threatened mostly by omitted variable bias in our models, and by causal ambiguities. The most critical factors that could impact the validity of this paper will be explained in the following.

Market timing effects

Previous literature on IPOs and SEOs has generally found that sponsors are better at timing exits when market valuations are high. Thus, one potential explanation why companies with financial sponsor-backing overperform, and companies where the financial sponsor has exited cease to overperform, may be that sponsors are better at timing exits. We use various control variables to reduce this effect, and most importantly control for exit year to capture the "hot market" effect previously found (Loughran and Ritter, 2002). Further, as the exit process usually involves multiple transactions for the sponsor, we would not expect the timing of each selldown to be at the top of the market. This would also go against our finding that sponsors put less emphasis on increase in valuation since IPO when timing selldowns, and the finding that sponsor-backed selldowns outperform in the aftermarket on a 1-year basis. Therefore, we do not consider timing effects a critical source of bias.

Unobserved company quality

It is possible that sponsor-backed companies in the sample have higher quality on average, which could explain their overperformance. Most critically, we do not include common measures of firm quality such as price/book ratio, leverage ratio, profitability and revenue growth that are likely to impact returns and discounts, potentially causing omitted variable bias. We do not have a way of controlling for this due to data limitations. However, ex-ante observable quality should be priced into the share already at the IPO. In addition, we argue that by controlling for size, country, sector and valuation change since the IPO we mitigated this issue to a certain extent. Size, country and sector should capture a lot of the variation in e.g. leverage and valuation multiples, and valuation change is likely to capture potential momentum effects that has proven to be associated with abnormal performance.

Blockholder effects

In general, we find that sponsor-backed companies outperform non-sponsored companies in the sample. It is possible that some of the sponsor-backing effect is explained by having any blockholder. Specifically, this effect would be likely to come from increased monitoring due to the absence of the free-rider problem (Berk and DeMarzo, 2017). As we only include companies that do selldowns, it is highly unlikely that any company within the sample has no blockholders as a selldown is, by definition, undertaken by a blockholder. However, for clean-up hypotheses specifically, it may be the case that we are simply estimating the effect of the last blockholder exiting the company. Thus, the effect we find on longer-term returns could be attributable simply to the company no longer having a blockholder, rather than having no sponsor-backing. However, this would not explain why markets generally react positively to a clean-up trade as measured by first-day returns.

Earnings management

We find positive 1-year abnormal performance for sponsor-backed selldowns. Additionally, we see the effect disappear within one year of the clean-up trade. This could potentially be caused by "window dressing" by sponsors leading up to selldowns. In other words, sponsors could be better at making accounting performance peak around the time of the selldown. We have no reliable way of determining if this is the case or not, but it should be noted that previous literature has found evidence supporting this (Hopkins and Ross, 2013). Again, the nature of the exit process with selldowns suggests that this is unlikely to be the case. The exit process typically runs over multiple years, starting at the IPO and ending at the clean-up trade. Window dressing would be very difficult to keep going for a period this long, as markets would likely realise the true performance along the way. That is, on average, the abnormal performance is less likely to stem from earnings management than is the case for e.g. an IPO.

8.4.2 Data reliability

The reliability of results is dependent on the quality and accuracy of the data we use when estimating effects. We have identified a number of potential flaws in our data collection and -use which will be explained in the following.

Lack of dividend adjustment

A shortcoming of data is the lack of dividend adjustments. This flaw relates to returns over longer time periods, but the impact should be minimal as the longest time period we consider is one year. The potential bias relates only to the abnormal part of performance, as returns are adjusted on basis of non-dividend adjusted price indexes. Thus, bias will only arise if differences in payout policy of sponsor-backed and non-sponsored companies are so substantial that it will have a material effect over the span of one year, or if the companies in the benchmark portfolio deviate in a similar way from the sample companies. Although we have no concrete evidence, we see this as being unlikely considering that sponsors, most often than not, have relinquished majority holding when the company goes public. Although the decision can be influenced in a number of ways, the sponsor is not free to set the payout policy of its portfolio companies after they go public.

Inaccurate estimation of lock-up periods

Data quality issues may influence results under the lock-up hypotheses. Some data-points on the existence and length of lock-up were missing in the main dataset from Dealogic, in which case we used Bloomberg to manually find lock-up data, effectively eliminating this issue. A more serious problem is that a few deals happened before lock-up expiry *without* Dealogic indicating that the lock-up was waived. In instances where no evidence to the contrary was found, we assumed that the lock-up was in fact waived, and set days between lock-up expiry to deal to zero. We do not regard this as a material issue, as the problem only applies to a relatively small number of observations. However, this method does come with the risk of incorrectly identifying an immediate selldown.

Erroneous classification of sponsor-backed and non-sponsored deals

We relied on Dealogic's classification of owner type. Specifically, deals identified as *financial sponsor related* were included in the sample as sponsor-backed deals. The potential problem is that the binary classification does not distinguish between deals where the financial sponsor is the sole owner and deals where the financial sponsor owns only a minority position. It also makes the division between financial sponsors and other owner types more black and white than justified by reality. In some cases, active, financial owners may be identified in the group we otherwise associate mainly with family- and corporate owners, because they do not fit the financial sponsor classification. In most cases, Dealogic identify the selling shareholders. We have manually checked if the classification was correct where selling shareholders were identified, and found no evidence of incorrect classification.

Geographic distribution of the sample

The sample is very unevenly distributed across countries, with especially the U.S. making up a large part of the sample. This is likely due to the U.S. simply having more ECM and private equity activity than other countries in the sample. While we do control for country-specific effects by including country dummy controls in all regressions, the results are still likely to be driven more by some geographies than other. Therefore, the results apply to developed market selldowns in general, but we are unable to say if the results hold for individual countries in the sample.

Misspecification of reference point for valuation

When controlling for valuation change, we use the change in market capitalisation between IPO and selldown. In order to make results more robust, we should include pre-IPO valuation metrics, e.g. the price at which the financial sponsor acquired the company. It is likely that sponsor-backed companies has seen their value increase more leading up to the IPO than have non-sponsored companies. Thus, this would have an effect on the models predicting the change in probability of an instant selldown if we do not capture the actual valuation increase the owner has seen, but only the part since the IPO.

Lack of control for non-sponsored clean-up trades

We do not control for the effect of clean-up trades undertaken by non-sponsors. This issue relates to hypothesis group 1 and 3, as we do not include non-sponsored transactions in group 2. In some models, we control for clean-up trades, but can only do so for sponsored selldowns. This could potentially skew results, as we have shown strong effects of clean-up trades for sponsors. There are a number of reasons, however, why we do not consider this as a larger threat to the validity of results. First, the clean-up trade effect is likely to be less pronounced for non-sponsors. This is due to non-sponsors taking a longer time to exit through selldowns, making the share overhang effect less imminent and thus less likely to depress market prices. It is also harder for markets to determine whether a trade is a clean-up as non-sponsors are more likely to hold on to shares for long-term investments. Second, if we were to control for clean-up trades for non-sponsors, we argue that this would, for the most part, strengthen the effects we find. In hypothesis 1a, controlling for the positive clean-up trade effect (if it exists for non-sponsors) would capture positive variation with discounts for non-sponsored transactions, and likely make the effect of sponsor-backing alone increasingly positive. In hypothesis 1b, it is questionable whether the effect exists as abnormal 1-year performance for non-sponsors is close to 0 on average, meaning that there is no positive abnormal performance to revert. In hypothesis group 3, we do not control for clean-up for either owner type. This could bias some of the results to a lesser extent if the clean-up trade effect is weaker for non-sponsors as we argue it should be. However, these hypotheses set out to test differences between sponsors and non-sponsors, regardless of the nature of the selldown. Thus, we are not interested as such in attributing some of the effect to clean-up trades, but rather to consider effect on average for each group of owners.

8.5 Implications for practitioners

We reinforce existing theory by extending previous studies on IPOs and SEOs specifically to selldowns. Our findings are in line with the literature, finding that sponsor-backed selldowns have smaller discounts. An investor can use this knowledge to guide his expectations of the level of discount when presented with the opportunity to invest in a selldown. This knowledge might also improve the institutional investor's ability to compare discounts of sponsor-backed and non-sponsored selldowns, respectively. While theory tells us that sponsor-backed IPOs outperform non-sponsored IPOs in the aftermarket, our findings add an important distinction: Overperformance is not fully priced into the share in the immediate aftermarket following the IPO, but remains for the selldowns. All else equal, sponsor-backed selldowns remain more attractive to investors than non-sponsored selldowns in the long run, despite the smaller discount. Based on our results, given the choice between two selldowns that are identical except for the fact that one is sponsor-backed and the other is not, the long-term investor should choose the sponsor-backed selldown. On average, a sponsor-backed, non-clean-up selldown gives 1-year abnormal returns of about 9.5%, while a similar non-sponsored selldown returns 2.3%. In other words, the investor gains 7.2 percentage points of abnormal performance, on average, by choosing the sponsor-backed over the non-sponsored selldown. It is worth noting that both sponsor-backed and non-sponsored selldowns, on average, have positive abnormal returns on a 1-year basis. On the other hand, an investor who intends to "flip" (immediately sell) his allocation should favour non-sponsored selldowns, as these come with greater discounts. A sponsor-backed, non-clean-up selldown returned abnormal 0.6% between offer and first-day close, while non-sponsored selldowns yielded 1.6%. Here, the investor would stand to gain an extra 1 percentage point of abnormal returns in a single day on average by choosing non-sponsored selldowns.

Part two of the paper considers what happens when the financial sponsor exits the investment at the clean-up trade. To our knowledge, no prior research exists on the topic. First, we find that the clean-up effect offsets the positive effect of sponsor-backing on a 1-year basis. Second, we find that discounts are smaller and first-day returns better. Thus, an investor should exercise caution when the block that is up for sale is the last one that the sponsor holds. The investor will get a smaller discount, but a better first-day return, an effect which he needs not participate in the transaction to be affected by. 1-year returns, however, are comparable to companies that were not sponsor-backed in the first place. If our view of the underlying reason for the short-term effects of the clean-up trade holds, we believe that investors put too much emphasis on the threat of liquidity events and too little on the benefits of financial sponsor-backing. To the practitioner, expected holding period will be an important factor: our findings show that it is more attractive to participate in a clean-up selldown if the investor "flips" his allocation than compared to holding it for the long-term. Doing so will give a one-day abnormal return of 1.8%, which is even better than for non-sponsored selldowns. Even if the investor intends to hold the investment for a year, a sponsor-backed clean-up trade should be preferred to a non-sponsored selldown, as it yields approximately 5.6% abnormal returns, 3.3 percentage points more than an average non-sponsored selldown.

Table 20: Comparison of sample average returns when investing in a selldown (unconditional mean values)

	Discount	**1-day**	**1-year**
Non-clean-up			
Sponsor	-3.6%	0.6%	9.5%
Non-sponsor	-4.3%	1.6%	2.3%
Total	-3.9%	1.1%	6.2%
Clean-up			
Sponsor	-3.3%	1.8%	5.6%

Examining the signaling value of transaction timing in relation to lock-up expiry, we get mixed results. In the sample, we find strong evidence that financial sponsors sell down sooner than non-sponsors, and they are also much more likely to sell down immediately after their lock-up provision expires. We also find that non-sponsors are more likely to do an immediate selldown when the share has shown strong performance since the IPO. However, we do not find evidence that markets react differently when non-sponsors choose to sell down immediately compared to sponsors. This is in contrast to our reasoning for why selldowns shortly from lock-up expiry for non-sponsors should be received negatively by markets compared to immediate selldowns for financial sponsors. Therefore, this last hypothesis group offers some interesting points on the effect of incentives on selldown timing, but little actionable advice to practitioners.

8.6 Implications for further research

Our main contribution to the literature, we believe, is the findings relating to clean-up trades. We see these as being particularly interesting as we find strong effects in line with our hypotheses, on a topic where no previous literature exists. Ideally, further research would make up for some of the shortcomings of this paper. For these hypotheses, it would be interesting to include non-sponsored selldowns to see if the effect hold for other owner types.

We failed to find evidence supporting the hypotheses relating to the signaling value of the instant selldown, despite finding strong evidence supporting the hypothesised motives behind selldown timing. Future studies could attempt to consider only surprise announcements in trying to isolate the signaling effect. This is likely to reduce noise and isolate the signaling effect, as would be ideal in testing these hypotheses.

We find positive abnormal 1-year returns for both owner types in our sample if the investor buys at the selldown offer price. Given the scope of this paper, we have no indication of why this is the case. Whether this is robust to other time periods, geographies and return adjustments is unclear, but would be relevant to explore further. The abnormal returns are less significant if we adjust for discounts, and measure returns from last trade before the offering. However, it would be relevant to consider whether these patterns of abnormal returns, especially for sponsor-backed selldowns, are likely to be found in the future, or if they simply occur by chance or biased regressions in this paper.

In general, as research on the topic of selldowns is very limited, the robustness of results can be called into question until other approaches have been applied. First, adding operating performance metrics as control variables would decrease the likelihood of the effect being caused by omitted variables. Second, whether results hold for other geographies and periods would be a valuable addition to the literature.

9 Conclusions

In line with established IPO literature, we find smaller discount and better 1-year abnormal performance for sponsor-backed selldowns compared to non-sponsored selldowns. Our models suggest that sponsors get about 0.3 to 0.6 percentage points smaller discount when doing a selldown. These findings are consistent with the certification effect, stating that, due to repeated interaction with capital markets, financial sponsors are seen by investors as more credible, reducing the need for discounts. We also find that some of the difference in discounts may be caused by the effect that occurs at the clean-up trade. Controlling for clean-up trades, the difference in discounts between sponsors and non-sponsors becomes much less significant, statistically as well as economically.

Testing the effect of clean-up trades specifically, we find that clean-up trades do indeed have smaller discounts compared to other sponsor-backed selldowns. We attribute this mainly to the share overhang being eliminated, but it may also be affected by increased demand as the clean-up trade is the last chance for investors to buy a larger block of shares without pushing up the share price. Interestingly, we also find a negative impact on 1-year abnormal performance after the clean-up trade. We interpret this as being the positive effects of sponsor-backing disappearing as the sponsor ceases to influence operations, but it remains a possibility that some of the effect is caused simply by earnings management or market timing by the sponsor.

Finally, we show that timing decisions differ for sponsors and non-sponsors; non-sponsors are more likely to sell when valuation has increased since the IPO, while sponsors seem to place less weight on that particular factor. This corresponds to the notion that sponsors are facing tighter time constraints compared to non-sponsors. We fail to find solid evidence proving that markets react differently to immediate selldowns after lock-up expiry for sponsors relative to non-sponsors, which we attribute partly to market participants having priced in the effect prior to the selldown.

The effects found throughout this paper imply that a short-term investor should favour non-sponsored selldowns and sponsor-backed clean-up trades, whereas a longer-term investor should invest in sponsor-backed selldowns regardless of whether it is a clean-up trade or not. If given the choice, sponsor-backed, non-clean-up selldowns far outperform other types of selldowns on a 1-year basis, while sponsored clean-up selldowns have the largest first-day return.

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